

This electronic thesis or dissertation has been downloaded from the King's Research Portal at <https://kclpure.kcl.ac.uk/portal/>



Towards a Mapping Account of Applicability

An Exposition, Explanation, and Justification of the Representational Conception of Applied Mathematics

Pointon, Daniel

Awarding institution:
King's College London

The copyright of this thesis rests with the author and no quotation from it or information derived from it may be published without proper acknowledgement.

END USER LICENCE AGREEMENT



Unless another licence is stated on the immediately following page this work is licensed

under a Creative Commons Attribution-NonCommercial-NoDerivatives 4.0 International

licence. <https://creativecommons.org/licenses/by-nc-nd/4.0/>

You are free to copy, distribute and transmit the work

Under the following conditions:

- Attribution: You must attribute the work in the manner specified by the author (but not in any way that suggests that they endorse you or your use of the work).
- Non Commercial: You may not use this work for commercial purposes.
- No Derivative Works - You may not alter, transform, or build upon this work.

Any of these conditions can be waived if you receive permission from the author. Your fair dealings and other rights are in no way affected by the above.

Take down policy

If you believe that this document breaches copyright please contact librarypure@kcl.ac.uk providing details, and we will remove access to the work immediately and investigate your claim.

This electronic theses or dissertation has been downloaded from the King's Research Portal at <https://kclpure.kcl.ac.uk/portal/>



Title: Towards a Mapping Account of Applicability

An Exposition, Explanation, and Justification of the Representational Conception of Applied Mathematics

Author: Daniel Pointon

The copyright of this thesis rests with the author and no quotation from it or information derived from it may be published without proper acknowledgement.

END USER LICENSE AGREEMENT



This work is licensed under a Creative Commons Attribution-NonCommercial-NoDerivs 3.0 Unported License. <http://creativecommons.org/licenses/by-nc-nd/3.0/>

You are free to:

- Share: to copy, distribute and transmit the work

Under the following conditions:

- Attribution: You must attribute the work in the manner specified by the author (but not in any way that suggests that they endorse you or your use of the work).
- Non Commercial: You may not use this work for commercial purposes.
- No Derivative Works - You may not alter, transform, or build upon this work.

Any of these conditions can be waived if you receive permission from the author. Your fair dealings and other rights are in no way affected by the above.

Take down policy

If you believe that this document breaches copyright please contact librarypure@kcl.ac.uk providing details, and we will remove access to the work immediately and investigate your claim.

Towards a Mapping Account of Applicability:

An Exposition, Explanation, and Justification of the Representational Conception of Applied Mathematics

Daniel Pointon

(S/N 0531893)

Kings College London

Department of Philosophy

Submitted in Partial Fulfilment of the
Requirements for the Degree of Doctor of Philosophy

I declare that the work presented in this thesis is entirely my own.

.....

Daniel Pointon

17.02.2012

Abstract

This thesis defends the view that the role of applied mathematics is a representational one, and develops a mapping account of the applicability of mathematics that does justice to this representational conception. The first chapter outlines some philosophical problems of applicability and some of its history. In the second chapter I explain in detail what the mapping account is, examining mappings, and representation theorems, and give any account how mathematics can represent derived attributes and laws. In chapter three I argue against the possibility of genuine platonistic explanations of physical phenomena. This is necessary as if there were such explanations they would entail that platonistic mathematics is not extrinsic to what actually goes on in the physical world, and that a purely representational conception of the applicability of mathematics is either straightforwardly false or radically incomplete. In chapter four a positive proposal, based largely upon the work of Hartry Field, is given for showing how it is that we can state scientific theories in such a way that platonistic mathematics does not appear as part of scientific theories. This is essential, since although the previous chapter argued that such genuine explanations are impossible, it did not show positively how we could dispense with platonistic mathematics in scientific explanations. Chapter five concerns a philosophical problem of applicability, the 'descriptive problem of applicability' which, it has been argued, goes beyond mere 'representational' issues and poses a problem for the mapping account of applicability. I identify three species of descriptive problems and reject the possibility of anthropocentrism as a solution to the descriptive problem. I then provide a solution to each of the three species of descriptive problem, concluding that the descriptive problem is not an issue for the mapping account of applicability. In chapter six I explore some implications of the mapping account for wider issues in the philosophy of mathematics. These issues include the indispensability argument for mathematical platonism; whether the mapping account is committed to abstract mathematical objects; the extent to which the account can be nominalised; and whether idealisations pose any problems for the mapping account. I argue that the account is *prima facie* committed to abstract objects but that there are construals of the account which are both acceptable and useful to a nominalist. Turning then to the issue of idealisations, I argue that neither physical nor so-called mathematical idealisations pose any problem for the mapping account, in the former case because they do not, properly understood, go beyond representation, and in the latter case because such idealisations, if they exist, are not part of the actual explanations of empirical phenomena. I conclude therefore that the role of applied mathematics is a purely representational one, and that the mapping account is a powerful and persuasive theory of that representational role.

Acknowledgements

I am grateful to the AHRC for a fifteen month full-time studentship, during which I wrote the bulk of this thesis, and to Kings College London for a Teaching Fellowship that enabled me to complete most of the remainder. Thanks go to Professor David Papineau who was my supervisor for the period of writing the thesis, and whose careful reading of the chapters, identification of errors, and generous insights were invaluable. Any errors remaining are entirely my own. Thanks also to Dr. Michael Gabbay, my previous supervisor, for his support in the period before I began the thesis itself. I am grateful to my friends for listening to me go on about this for the last few years, coming to talks and readings thesis drafts, and to Sarah who has cheerfully put up with many weekend plans being abandoned to make time for writing, and who kindly got the thesis bound.

Contents

1. Introduction.....	7
<u>1.1 Chapter Introduction.....</u>	8
<u>1.2 What are the Philosophical Problems of Applicability?.....</u>	10
1.2.1 The Semantic Problem of Applicability.....	11
1.2.2 The Metaphysical Problem of Applicability.....	14
1.2.3 The Descriptive Problem of Applicability.....	16
1.2.4 The Historical Awareness of the Problems of Applicability.....	17
<u>1.3 Thesis Contents.....</u>	20
 2. Towards the Mapping Account: Representation, Measurement and Laws.....	24
<u>2.1 Chapter Introduction.....</u>	25
<u>2.2 The Mapping Account.....</u>	26
2.2.1 Measurement Types, Procedures, and Representation Theorems.....	28
2.2.2 Concatenation.....	33
2.2.3 What Needs to Be Mapped? Choosing a Domain and a Mapping.....	38
2.2.4 Selecting the Axioms for Extensive Measurement.....	42
2.2.5 Concluding the Mapping Account.....	49
<u>2.3 Mathematics in Natural Science.....</u>	51
2.3.1 Initial Conditions, Empirical Units, and Fundamental and Derived Attributes.....	54
2.3.2 Derived Attributes and Natural Laws.....	58
2.3.3 An Example of Mathematics Applied to a Law.....	61
<u>2.4 The Inferential Account.....</u>	66
2.4.1 What is the Inferential Account?.....	66
2.4.2 Putative Advantages of the Inferential Account over the Mapping Account.....	68
2.4.3 Remarks Concerning the Inferential Account.....	76
 3. The Dispensability of Platonistic Explanations of Physical Phenomena.....	77
<u>3.1 Chapter Introduction.....</u>	78
<u>3.2 Explanation and Genuine Platonistic Explanation.....</u>	79
3.2.1 Causal Explanations.....	81
3.2.2 Platonistic Explanations of Platonistic Facts.....	83
<u>3.3 A Candidate for a Genuine Platonistic Explanation?.....</u>	86
<u>3.4 Arguments Against the Cicada Example.....</u>	91
3.4.1 Arbitrariness.....	91
3.4.2 Begging the Question.....	93
3.4.3 Dispensing with Platonistic Mathematics: A Objection to Baker.....	95
 4. Eliminating Mathematics From Natural Science.....	104
<u>4.1 Chapter Introduction.....</u>	105
<u>4.2 Field's Nominalisation of Newtonian Gravitational Theory.....</u>	106
4.2.1 The Background to the Fieldian Programme.....	107
4.2.2 Nominalising Newtonian Space-Time and Magnitudes of Scalar Attributes.....	108
4.2.3 Nominalising the Gravitational Theory and its Constituent Concepts.....	116
4.2.4 Field's Nominalisation and the Mapping Account.....	126
<u>4.3 Balaquer's Nominalisation of Quantum Mechanics.....</u>	128

5. The Descriptive Problem of Applicability and its Solution	134
<i>5.1 Chapter Introduction.....</i>	<i>137</i>
<i>5.2 The Descriptive Problem of Applicability.....</i>	<i>128</i>
5.2.1 Pythagorean Reasoning as a Source of the Descriptive Problem	129
5.2.2 Three Examples of Pythagorean Reasoning	143
5.2.3 The Taxonomy of the Descriptive Problem.....	148
<i>5.3 Solutions to the Descriptive Problem</i>	<i>151</i>
5.3.1 Anthropocentrism as the Solution to the Descriptive Problem.....	151
5.3.2 Solving Descriptive Problems of the First Category.....	156
5.3.3 Solving Descriptive Problems of the Second Category	159
5.3.4 Solving Descriptive Problems of the Third Category	162
 6. Implications for Philosophy of Mathematics: Indispensability, Nominalism and Idealisation	168
<i>6.1 Chapter Introduction.....</i>	<i>169</i>
<i>6.2 Platonist Ontology and Indispensability.....</i>	<i>170</i>
<i>6.3 Can We Nominalise the Mapping Account?.....</i>	<i>174</i>
6.3.1 A Scholarly Point About Rizza and Field	176
6.3.2 The Fictionalist Approach to the Mapping Account	178
6.3.3 Nominalistically Acceptable Surrogates I: Rizza's Approach	187
6.3.4 Nominalistically Acceptable Surrogates II: Hellmans's Approach	194
<i>6.4 The Role of Idealisations</i>	<i>198</i>
6.4.1 Categorising Idealisations	199
6.4.2 Mathematical Idealisations, Batterman, and the Mapping Account	202
6.4.3 The Mapping Account and Physical Idealisations	208
6.4.4 Idealisations in Field and Rizza	211
<i>6.5 Thesis Conclusion</i>	<i>213</i>
 Bibliography	216

Chapter 1

The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve.

– Eugene Wigner (1960)

Introduction

The purpose of this chapter is to orientate the reader with the questions in the philosophy of applied mathematics; with some of its historical background; with the debates which will prove central to the development of the overall thesis; and with the direction the thesis will take. Section 1.1 introduces the need for a robust philosophy of applied mathematics and explains that the philosophical account developed will be the *mapping account*. Section 1.2 discusses philosophical problems of applicability, drawing on the work of Mark Steiner, a key contributor to this area, and specifies which philosophical problems of applicability in particular I shall be focusing on. I then consider how the applicability of abstract mathematics became a philosophical issue in the nineteenth and twentieth centuries, providing the reader with an overview of what has led to the current debate. The final section, 1.3, outlines what I shall be arguing in this thesis, the subjects of the chapters to follow.

1.1 Chapter Introduction

In this thesis I shall construct a representational account of the applicability of mathematics to empirical phenomena, and argue that this account, the *mapping account*, explains all of the empirical applications of mathematics. That is, I shall develop the mapping account and claim that the applicability of mathematics to empirical descriptions, predictions and explanations, is purely a matter of the representational capacities of mathematics. If I am successful then negative claims about our ability to explain applicability, similar to those expressed by Wigner in the epigram above, will be refuted. Mathematics is important to science because it is so useful in precisely representing magnitudes of attributes of empirical objects, and the relations of these attributes, with a large amount of abstraction, ignoring features of those objects that are not relevant to the attributes being described. The account I defend holds that mathematics is able to exercise such a representational role because of *mappings* from empirical to mathematical structures. Constructing an account of applicability is not an easy task, although many of the elements of such an account may appear to be a matter of common sense once the problems involved are fully elucidated.

The philosophy of mathematics has an ancient provenance, with contributions from figures as venerable as Plato, Aristotle and Pythagoras, and it is therefore hardly surprising that a great deal of philosophy of mathematics is described as Platonist, that Aristotelian discussions concerning abstraction should abound, or that Pythagorean concerns such as the nature and role of mathematical objects are fiercely debated. For the most part the debates in such areas have centred on issues of mathematical ontology, mathematical knowledge and mathematical truth – to the extent that the philosophy of applied mathematics has been comparatively neglected, especially where questions of applicability as *sui generis* philosophical questions have been concerned. Why should this be so? I think one very important reason is that it was felt that if mathematical knowledge could be explained there would be little to say about how mathematics could be applied, and

that if applications such as counting were then philosophically unproblematic there would be no good reason to think that other applications of mathematics should not be so. This broaches the issue of how concerns with applicability relate to traditional questions in the philosophy of mathematics, questions of ontology and epistemology. But to focus overly much on ontological concerns in the development of an account of applicability will only mean that one ends up focusing more on the question of what mathematical objects are at the expense of how it is they can be usefully applied. This is not to say that such traditional questions are irrelevant to applicability insofar as they concern how mathematical knowledge is possible, and what the nature of mathematical structures is, but *that* we have mathematical knowledge and that there are mathematical structures of some sort is assumed by the applicability theorist at the outset.

Applicability is a problem for both the platonist and the nominalist. This is evidently the case for the platonist – he has to explain how *abstract* mathematical structures can be applied to the *concrete* world. But the problem of applicability exists for the nominalist as well. For him the usefulness of mathematics stills need to be explained, even though as such he does not believe in the existence of abstract mathematical objects. But as I said above, a philosopher accounting for applicability does not need to explain what our mathematical knowledge is knowledge of, or how we have it, as he can to a large degree take this for granted, given that the practice of mathematics, and its successes, whatever its true subject matter, would challenge anyone to suggest otherwise. Thus, for the purposes of developing the mapping account I shall assume we have mathematical knowledge, but I will discuss some ontological concerns in chapter six however, concerning whether the mapping account is committed to any particular mathematical ontology; the indispensability argument for mathematical platonism; and the degree to which a nominalist can make use of the mapping account. Additionally some ontological issues are discussed *passim* in chapters three and four, e.g. whether certain mathematical concepts and scientific theories containing references to seemingly abstract mathematical objects can be nominalised, but this is in the particular context of

clarifying what the mapping account needs to explain, rather than ascertaining and endorsing a general metaphysics of mathematics.

The neglect of genuine questions of applicability in favour of questions of epistemology and metaphysics is surely due, at least in part, to the ubiquity and familiarity of many paradigmatic examples of the applicability of mathematics, with simple measurement, and especially counting, being amongst the most mundane. This way of thinking is very easy to fall into and so the philosopher of applied mathematics has to make a very detailed study of his subject matter in order to ensure that he does not mistake everyday familiarity for philosophical clarity. This is nowhere more often the case than with the mapping account of applicability. For once this theory is exposited to both the philosopher and the layman the response is frequently ‘that’s obvious’ or ‘that’s just common sense’, partly due to prior acquaintance with the construction of, and everyday engagement with, scales and measurements of various kinds. Unfortunately that it is not so obvious is substantiated by the facts that only recently has the mapping account emerged as a distinct and articulated philosophy, and that the elucidation of this account requires both a large quantity of complex theory to be engaged with, and tangled issues to be separated. This project is one aim of the thesis, but first I want to say a few words about the philosophical problems facing the philosopher of applied mathematics, and trace some of the recent history of attempts to address these philosophical problems. I shall then explain how this thesis fits together.

1.2 What are the Philosophical Problems of Applicability?

Intuitively the philosophical problem of applicability can be stated very simply: ‘how is it that we can usefully apply mathematics to the empirical world?’. This breaks down into two sub-questions, the two fundamental questions with which the philosophy of applicability is concerned: (1) how can mathematics be applied to the world *at all*? (2) how and why is it that mathematics can be applied *usefully*? I shall attempt to answer these questions in the chapters that follow by constructing a

mapping account of applicability. One philosopher to recognize the fact that there is not just a single philosophical problem of applicability is Mark Steiner, arguing (in a recent monograph, *The Applicability of Mathematics as a Philosophical Problem*) that there are in fact three such problems: the semantic, metaphysical, and descriptive problems. Although I believe, and argue in chapter five, that Steiner proceeds in a manner which is quite misleading as far as the fundamental questions of applicability are concerned, I shall present Steiner's taxonomy in this section because it has now entered the literature and become a relatively accepted way of classifying the philosophical problems of applicability. I shall indicate in what follows where I disagree from Steiner however, and hold that Steiner's metaphysical and descriptive problems are really just instances of the more general questions (1) and (2).

1.2.1 The Semantic Problem of Applicability.

As Steiner sees it, the semantic problem of applicability is the problem of how we can use mathematical language in applied mathematics, specifically whether number words are singular terms or predicates. He focuses on *natural* number words, although evidently the problem extends to real number words as well. The reason that we must get clear on the semantic problem before we can go further is because if we are making any sort of deduction our terms must be used consistently or else the deduction will not be valid. That the problem exists is evidenced by the different uses of number words, e.g. 'there are ten pens on my desk' and 'ten is a prime number'. The problem can be illustrated via a simple deduction:¹

- a. There are ten pens on my desk
- b. There are five books on my desk
- c. No pen is a book
- d. There is nothing else on my desk

¹ Based on that given by Steiner (1998, p.16)

- e. $10 + 5 = 15$
- f. There are fifteen items on my desk.

The problem is that in **a**, **b**, **f** numbers appear as predicates like ‘the pens on my desk are ten’, yet in **e** the numbers appear as singular terms.² This is troubling since all the premises are needed for the conclusion to be derived but we cannot simply move across from predicates to singular terms and back again – this is analogous to allocating different values to the same variable in the scope of a single sub-proof before that sub-proof is discharged, we cannot treat ‘fifteen’ as a predicate one moment and a singular term the next and then move back to ‘fifteen’ as a predicate. So the derivation **a-f** appears to be invalid.³ Fortunately the semantic problem of applicability can be fairly easily resolved, at least in the case of arithmetic. To resolve the ambiguity a decision needs to be made about approach we shall adopt, namely treating number words as singular terms or as predicates. The first option has been pursued fairly extensively and so I shall consider it here. There are two strategies in particular I wish to consider, the Fregean strategy and the set-theoretic strategy.

Frege’s idea was that we talk about numbers belonging to various concepts. For instance “If I say ‘the King’s carriage is drawn by four horses’ then I assign the number *four* to the concept ‘horse that draws the King’s carriage’” (Frege 1884, p.59). The deduction **a-f** can be formalised in second-order logic using the following formula:

$$[(\text{NxFx} = m) \wedge (\text{NxGx} = n) \wedge (\sim \exists x(\text{Fx} \wedge \text{Gx}))] \supset (\text{Nx}(\text{Fx} \vee \text{Gx}) = m + n)$$

² Note that at this stage the issue is not ontological – the use of singular terms is not supposed to suggest platonism in mathematics, the reference could equally well be to a nominalistically acceptable surrogate for an abstract object, or to a useful fiction that is part of the ‘story’ of mathematics.

³ The problem would remain even if we were talking about predicates of *mathematical* objects, since the issue is with the predicate/singular-term distinction, not the abstract/concrete distinction.

With appropriate substitutions for variables and predicates, such that Nx means 'the number of x 's such that', Px means ' x is a pen' and Bx means ' x is a book', **a-f** becomes:

$$[(NxPx = 10) \wedge (NxBx = 5) \wedge (\sim \exists x(Px \wedge Bx))] \supset (Nx(Px \vee Bx) = 10 + 5)$$

Frege's strategy was guided by his metaphysics of concepts and objects, where monadic predicates denote concepts which objects may instantiate, or in Fregean terminology, 'saturate'. Frege was very emphatic that

...we should not be deterred by the fact that in the language of everyday life number appears also in attributive [i.e. as a predicate] constructions. That can always be got round. For example, the proposition that 'Jupiter has four moons' can be converted into 'the number of Jupiter's moons is four'. Here the word 'is' should not be taken as a mere copula, as in the proposition 'the sky is blue'. This is shown by the fact that we can say 'the number of Jupiter's moons is the number 4, or four'. Here 'is' has the sense of 'is identical with' or 'is the same as'. So that what we have is an identity, stating that the expression 'the number of Jupiter's moons' signifies the same object as the word 'four'. (Frege 1884, p.69)

The set-theoretic approach is not dissimilar to Frege's in many of its essentials. It would be endorsed by those who distrust intensional entities such as concepts and their attendant unfamiliar properties of saturatedness and unsaturatedness, and prefer sets with their clear-cut identity conditions. Rather than referring to numbers that belong to concepts we talk about numbers that are the cardinalities of sets (cardinal numbers). So we would for instance say that the cardinality of the set of horses that draw the king's carriage is 4. That is $|\{x : x \text{ draws the King's carriage}\}| = 4$. The members of set x will be in a 1:1 correspondence with all other 4-membered sets. Thus in formalising the above inference, we would say

$$[X = \{x : Px\} \wedge Y = \{y : By\} \wedge (|X| = 10) \wedge (|Y| = 5) \wedge (\sim \exists z(z \in X \wedge z \in Y))] \supset \\ |X \cup Y| = 10 + 5$$

That is, if X is the set of pens and Y is the set of books and the cardinality of X is 10 and the cardinality of Y is 5 and X and Y are disjoint then the cardinality of the union

of those sets will be $10 + 5$, i.e. 15. It is therefore clear that the possession of a robust method for construing all statements of cardinal number as involving singular terms rather than predicates is effective at removing the ambiguity that may be associated with statements of number, and that there a variety of solutions to the semantic problem which we may endorse, depending on our inclinations. There are other more nominalistically acceptable approaches as well, but due to the tangential nature of the semantic problem with respect to this thesis I shall not pursue them here. Will the metaphysical problem of applicability be as easily solvable as the semantic problem?

1.2.2. The Metaphysical Problem of Applicability

Steiner explains that “to go by recent philosophical literature...the chief problems about mathematical applicability are what we can call the ‘metaphysical problems’” (Steiner 1998, p.19). These problems are explained as “stem[ing] from a gap between mathematics and the world” (ibid.). Many philosophers and philosophically-inclined mathematicians treat the objects of mathematics as distinctly non-concrete, even to the point of denying them a location in space and time, and any causal powers whatsoever, which would suggest that mathematics isn’t *about* the physical world. Others, disinclined to believe in this view of mathematical objects, prefer to think of mathematics as being about objects which may not be so abstract, such as possible structures or sentence tokens. The point is that both of these groups of philosophers will require an account of how we apply mathematics, a point I made in the chapter introduction. To focus on there being a *gap* between mathematics and the world is to make the metaphysical problem a problem for the platonist, and obscures the fact that the question of how mathematics can be applied is a problem for both the mathematical platonist and the mathematical nominalist. Additionally, to introduce ontological concerns at this juncture only clouds the issues, and introduces epistemological problems that are properly speaking the subject of other areas of the philosophy of mathematics. For

the question is ‘given we have mathematical knowledge, how can we apply it?’ rather than ‘what is mathematical knowledge actually knowledge of, how can we have this knowledge, and if it is knowledge of abstract objects how can we apply it?’.

However, because Steiner speaks of a gap between mathematics and reality it is clear that he takes this to be an especial problem for the platonistically inclined philosopher, an instance of the general question ‘how can mathematics be applied to the physical world?’, the ‘first question’ of applicability. Thus the metaphysical problem concerns how it is that if mathematics is the study of non-spatial, non-temporal, and acausal objects, how can we apply it to the spatial, temporal, causal world? This problem has been the focus of much philosophical thinking, and Steiner believes that Frege solved the problem of how mathematics can be applied for the platonist. (I shall only mention this briefly here as my concern is with the mapping account and not with Frege). Frege’s idea is that concepts are the bridge between abstract and concrete objects. Objects, including concrete objects, satisfy, or in Frege’s terminology, saturate, first level concepts. Numbers are the (abstract) objects that are the extensions of certain second-level concepts, which are concepts saturated by lower-level concepts, which are in turn satisfied by objects. Specifically numbers are the extensions of number, or numerosity, concepts such as the concept ‘equinumerous with the concept ‘pen in the pot’’. So numbers are related to concrete objects in virtue of being the extensions of second-level concepts which are saturated by first-level concepts whose extensions contain concrete objects.⁴

My own approach is very different to that of Frege as I argue that mathematics is applicable not because mathematical and concrete objects are related via concepts, but rather because the mathematical can *represent*, through mappings, the empirical, appealing to a fair amount of measurement theory to explain in detail how these representations obtain, since measurement theory concerns the properties that empirical structures must possess in order for a mapping to be possible. At this stage the mapping account is not obviously any more pro-platonist than anti-platonist, and as I have said, I am largely silent on

⁴ See Frege (1884) and (1892).

ontological matters until chapter six. In chapter two I try to do justice to the mapping account of applicability but suffice it to say that if the mapping account is successful it will solve the first fundamental problem of applicability, and, *a fortiori*, that instance of it that is the metaphysical problem of applicability.

1.2.3. The Descriptive Problem of Applicability

The descriptive problem of applicability concerns the fact that mathematics, the subject matter of which is commonly taken to be abstract and causally impotent, plays a major role in the description and discovery of seemingly true physical theories, and the prediction of novel physical phenomena. As such it is plainly an instance of the second problem of applicability as outlined above, namely ‘how and why can mathematics be usefully applied?’ since to account for the role of mathematics in discovery and novel prediction is to account for (part of) its use. Steiner feels that the problem is that our mathematical knowledge enables us to gain natural-scientific knowledge in a way which appears to go beyond any explanation of applicability as just a matter of the utilisation of the representational capacities of mathematical structures to greatly simplify the making of physical predictions and statement of physical theories. Steiner argues that the descriptive problem primarily arises from Pythagorean reasoning, a form of reasoning that derives novel or unexpected empirical results from purely mathematical results. The most prevalent example of this is claimed to be Pythagorean analogy, which occurs when we have an...

...equation E [which] has been derived under assumptions A [and] the equation has solutions for A which are no longer valid; but *just because they are solutions of E*, one looks for them in nature...the analogy becomes Pythagorean if [two solutions] are physically *disanalogous*, so that *only* the equation links them. (Steiner 1998, p.76).

Another form of analogy identified is ‘formalist analogy’, although Steiner argues that formalist analogy itself is properly a species of reasoning that falls under the Pythagorean genus. When making formalist analogies, physicists take previous

formalisms and extend them, subject to certain formal restrictions, hoping that the formal restrictions will cash out empirically and that the extension of a previously successful formalism will yield an equally successful formalism in a new case. Putative examples of Pythagorean analogy include Maxwell's prediction of electromagnetism as a result of his modification of Ampere's laws, and Schrödinger's discovery of wave mechanics. In opposition to this, I shall argue in chapter five that Steiner is wrong, that the problems of surprising predictions and novel discoveries, among others, are not categorially different to the problem of explaining how mathematics assists in making more mundane predictions and discoveries, a role I argue is fully explained by the mapping account.

1.2.4. The Historical Awareness of the Problems of Applicability

In the late nineteenth and twentieth centuries the problems raised by mathematical applicability began to be recognized, where they had earlier been obscured by the lack of a principled distinction between pure and applied mathematics. The problems of applicability as I consider them in this thesis were not really discussed before this time, for although Aristotle, Kant and Frege did consider some issues of applicability, they were much more concerned with geometry, cardinal arithmetic and metaphysics than with measurement and representation. The philosophy of mathematics in the first half of the twentieth century was distinguished by its almost exclusive focus on issues in the philosophical foundations of mathematics, specifically with the avoidance of the various set-theoretic paradoxes, the problems posed by the concept of infinity, and the nature of mathematical knowledge. However work on applicability did take place, although any history of applicability in the twentieth century has to largely bypass the three great schools, with the exception that in the *Principles of Mathematics*, Russell does make some mention of measurement, a concern not followed through to a great extent in *Principia Mathematica*, where it receives a relatively brief treatment in part six of volume three.

In the late nineteenth century, however, concern with the applicability of mathematics and the representational conception of mathematical applicability – the conception with which I am concerned here – was manifested in early work on the theory of measurement. A significant amount of contemporary work concerning the application of mathematics to empirical phenomena, and certainly a large part of this thesis, involves the theory of measurement, and it is in this mathematical discipline rather than in the philosophical schools that much work necessary to arriving at a proper theory of the applicability of mathematics was carried out. Much measurement theory is concerned with what is known as ‘representational measurement’, namely how mathematics can represent empirical attributes so that these attributes can be measured. Representational measurement is therefore concerned with the properties that empirical systems need to possess in order to be measurable in a certain way by a mathematical or numerical system. Who was responsible for the development of the theory of representational measurement? David Hand explains:

The approach began its formal development around the end of the nineteenth century and the beginning of the twentieth century with the work of Helmholtz (1887), Holder (1901) and Russell (1903), although the ideas can be traced to Euclid and beyond. Helmholtz gave conditions of order and combination which objects must satisfy for these relationships to be represented by order and addition of positive real numbers...and Holder developed these ideas further. (Hand 2004, p.26).

There was some discussion of measurement-related issues in ancient Greek philosophy, and Eudoxus’ *Theory of Proportions*, which appears in Euclid’s *Elements*, is often taken to be the core of that discussion. However it was not really until the last century that mathematicians and physicists became interested in issues of measurement in their own right, when Hermann von Helmholtz published his ‘Epistemological Analysis of Counting and Measurement’ and Otto Holder his ‘Axioms of Quantity and the Theory of Measurement’, the theorems of which were crucial to the development of measurement theory as its own branch of mathematics, culminating in the three volume *Foundations of Measurement* in 1971. That work is the standard text in the field and one which furnishes a significant

amount of material for this thesis. At any rate, the late nineteenth and early twentieth century has been the scene of a huge amount of work concerning the theory of measurement in general and representational measurement in particular, with the task of really clarifying how measurement is possible, how it is that mathematical structures can represent empirical structures. (I am not going to say any more about representational measurement here as it is discussed in detail in the next chapter).

So we see that in the twentieth century rigorous mathematical characterisations of measurement became widely available, characterisations very useful to a philosopher thinking about philosophical issues of applicability. This brings us to contemporary *philosophy* of applicability, for I have hitherto only mentioned the *mathematical* theories of measurement. It is by no means an exaggeration to say that philosophy was, by and large, unconcerned with how mathematics could be applied until relatively recently. A quote from Hartry Field will substantiate this view:

Most of the literature in the philosophy of mathematics takes the following three questions as central: (a) how much of standard mathematics is true?... (b) what entities do we have to postulate for the truth of... mathematics...? (c) what sort of account can we give of our knowledge of these truths? A fourth question is also sometimes discussed, though usually quite cursorily: (d) what sort of account is possible of how mathematics is applied to the physical world? (Field 1980 p.vii).

This quote is apt, since there is a view that it is the work of Field that inaugurated the philosophical concern with applicability that has arisen over the past thirty or so years, a view I to some degree share, and so it is only natural Field's work should play a significant role in this thesis. That Field's work has engendered so much activity around the topic of applicability is slightly ironic, since his primary concern was not to develop a theory of applicability but rather to convince platonists of the dispensability of mathematics to science and to thereby motivate nominalism. Much of the work on the philosophy of applicability from the past decade has used an approach similar to that of Field, making measurement-theoretic concerns quite central. The mapping account of the applicability of mathematics thus owes a lot to

Field, and I agree with Michael Friedman that “future discussions of this area must take up where Field leaves off” (1981).

1.3 Thesis Contents

After this introduction, the thesis proper opens in chapter two with a thorough discussion of the mapping account of the applicability of mathematics. The first part of that chapter describes in detail what the account is, explaining how mathematical structures are able to represent empirical phenomena and discussing one example of the representational applicability of mathematics, measurement, in some detail. I look at the two main approaches to measurement, viz. measurement procedures and representation theorems, and outline the different types of measurement, before going on to explore what is meant by the empirical operation of concatenation, which is so important in measurement. I then examine the several types of mapping which the mapping account might claim is the ground of applicability, and the conditions under which an empirical structure can be *extensively* measured. The second part of the chapter moves away from measurement specifically, looking more generally at the role of mathematics in natural science, and giving a speculative illustration of how the applicability of mathematics in Newton’s law of universal gravitation can be fully accommodated by the mapping account, and clarifying the notions of fundamental and derived attribute, and the relations of the concepts of derived attribute and law, along the way. Finally I consider a representational account of the applicability of mathematics that claims to go beyond the version of the mapping account developed in this thesis, the *inferential account*, comparing the two accounts and isolating several flaws in the argument that the inferential account is an advance over the mapping account.

Chapter three is concerned with arguing against the possibility of genuine platonistic explanations of empirical phenomena. This is important as one aim of

this thesis is to justify the purely representational character of applied mathematics, a contention undermined if there are such genuine explanations. I give an outline of Alan Baker's example of a genuine explanation of a physical phenomenon involving platonistic mathematics, the cicada example, arguing that it does survive many of the arguments that have been developed to criticise it. I argue however that the platonistic concepts used in the example can be satisfactorily nominalised and so the example need not be a platonistic one, i.e. we need not think that the mathematics involved relies on abstract objects.

Chapter four is primarily an extended engagement with some of the methods of Hartry Field, in order to show *how* it is that we can state scientific theories with no reference to abstract mathematical objects, coordinate systems, or any other ontologically questionable or in-principle arbitrary entities. In order to do this I give a comprehensive account of Field's nominalisation of Newtonian gravitational theory, and show how Mark Balaguer tries to extend this to Quantum Mechanics. The work of this chapter is necessary in order to support the mapping account, for it is crucial to that account that the relation of mathematics to empirical phenomena be a representational one, and this chapter shows in detail how platonistic mathematics can be treated as merely a representational tool – though one which is nevertheless very useful, perhaps practically indispensable, for scientists.

Chapter five discusses a problem of applicability concerning applied mathematics putatively having a role which goes beyond representation, referred to above as the descriptive problem. Recall, this problem is that mathematics allegedly allows us to describe or predict empirical phenomena in ways we would not expect, and suggests there is a closer fit between mathematics and the world than merely that mathematics can usefully represent attributes of that world. The chapter covers the sort of reasoning that is central to the descriptive problem, namely Pythagorean reasoning, and gives examples of such reasoning. I reject the possibility of anthropocentrism as a solution to the descriptive problem, maintaining that there are actually three species of descriptive problem, and I consider solutions to the descriptive problems of the first, second and third category respectively, arguing that either each of these problems is amenable to solution by, or compatible with,

the mapping account – or that the putative problematicity arises from a misunderstanding. I claim therefore that the descriptive problem of applicability does not pose a challenge to a representational conception of applied mathematics.

In chapter six I explore some implications of the mapping account for wider issues in the philosophy of mathematics including whether the mapping account is committed to abstract objects, and its relation to the indispensability argument. I argue that the mapping account is *prima facie* committed to abstract objects, but that there are construals of the account which are both acceptable and useful to a nominalist. I look at both Field's fictionalism and the nominalist views of Davide Rizza and Geoffrey Hellman which try to provide surrogates for abstract mathematical objects by reinterpreting parts of it. I argue that if a nominalist wants to keep the mapping account in some form he had better opt for the reinterpretation since in the fictionalist case the mapping account's only real use is to undermine the indispensability argument for platonism. In the surrogate-nominalist case however we try to explain the applicability to the empirical world of some nominalistically acceptable substitutes for parts of platonistic mathematics. Turning then to the issue of idealisations, I argue that idealisations do not pose any problem for the mapping account. This is because physical idealisations can be fully explained by the mapping account, or at least their solution is consistent with that account, and because there are no such things, or at least no extant examples of such things, as non-physical idealisations which play a genuine role in empirical explanations. I claim that the best known such example, Batterman's, fails for a variety of reasons.

I conclude that if platonism is true then the mapping account is an excellent explanation of the applicability of mathematics, but that if abstract objects don't exist (and thus that platonism is false), then suitably reinterpreted the mapping account will provide an excellent account of some nominalistically acceptable platonistic-object surrogate to empirical phenomena. If however fictionalism is true then the mapping account's use is a negative one, undermining platonism – although the fictionalist is free to admit that if, counterfactually, there were abstract mathematical objects, the mapping account could explain their applicability. But whether one is a platonist or a nominalist the mapping account can be a substantial

contribution. The development of a robust account of applicability is a vital and essential part of both the philosophy of mathematics and the philosophy of science.

Chapter 2

...mapping accounts seek to explain the utility of mathematics...by demonstrating the existence of the right kind of map from a mathematical structure to some appropriate physical structure.

– Robert Batterman (2010)

Towards the Mapping Account: Representation, Measurement and Laws

This chapter is concerned with explaining in detail exactly what the mapping account is. Many philosophers have alluded briefly to such an account, but only recently have any more detailed versions emerged. The first part of this chapter focuses on explicating the account, and how it relates to measurement. The second part is concerned with how the mapping account meshes with natural science, specifically the measurement of initial conditions and the description of natural laws. I argue that the mapping account shows clearly both how mathematical structures can represent empirical phenomena, and why the role of mathematics in science need only be a representational one. In the third part of the chapter I discuss and confront an account that claims to go beyond the mapping account, the so-called inferential account of applied mathematics.

2.1 Chapter Introduction

The mapping account is an account of the applicability of mathematics to empirical phenomena. It has been invoked or discussed by a variety of philosophers and consists minimally in the view that applicability can be explained by an appeal to structure-preserving mappings between mathematical and empirical structures.⁵ The origins of the account lie in Field's *Science without Numbers*, where many details of such an account are developed, although, as noted previously, Field's concern was not really to develop a theory of applicability, but to convince platonists of the indispensability argument's failure to establish platonism. That was thirty years ago, and the mapping account has recently received a quite comprehensive statement at the hands of Christopher Pincock in his (2004b) paper 'A New Perspective on the Problem of Applying Mathematics', where it is called the 'structuralist account'.⁶ 'Structuralist account' here does not refer to the metaphysics of mathematics defended, in various guises, by Hellman (1989), Resnik (1997), and Shapiro (1997), but is rather another name for the mapping account itself insofar as that account is concerned with mappings between mathematical structures and structural features of the empirical world.

In section 2.2 I describe the account in some detail, specifically in connection with measurement theory. There is a close connection between measurement theory and the mapping account, indeed measurement theory might be viewed as a rigorous development of the idea that is the germ of the mapping account. This is because measurement theory is concerned with investigating the structural properties that empirical structures need to possess in order to be mapped into, or represented by, mathematical structures, and serves to provide many examples of mappings between such structures. Therefore no comprehensive discussion of the mapping account is going to be possible without a substantial amount of measurement theory, and if measurement theory did not already exist it would be

⁵ Cf. Pincock (2004a), (2004b); Bueno and Colyvan (2011).

⁶ Especially section 4, 'the structuralist account'.

necessary for the mapping account theorist to invent it. Fortunately since measurement theory is an independent branch of mathematics in its own right, predating contemporary philosophical concerns with applicability, the mapping account theorist is spared this onerous task. In section 2.3. I consider how the mapping account can explain how and why mathematics is useful not just in representing and measuring magnitudes of fundamental empirical attributes, but in describing the natural laws the search for which forms an important part, if not the most important part, of the business of science. In this section I look at an example of a scientific law, viz. Newton's law of universal gravitation, and present a speculative illustration to support my view that nowhere in either the development or statement of that law does mathematics play any role other than a representational one, which can be fully explained by the mapping account. This conclusion also supports the view that the mapping account is sufficient for explaining the applicability of mathematics to natural science in general. In section 2.4. I consider a rival theory of applicability, Otavio Bueno and Mark Colyvan's 'inferential conception'. This, they claim, is a significant extension and modification of the mapping account which is able to handle challenges putatively beyond the mapping account's reach, and which therefore comprises a challenge to the adequacy of that account. I argue below in 2.4 that the inferential conception is not a threat to the mapping account as outlined in this thesis, and that some of the reasons proffered for preferring the inferential conception are in fact reasons that count against it.

2.2 The Mapping Account

The key idea of the mapping account is very simple to state, as I did above in 2.1. It is that mathematics is applicable to the empirical world in virtue of structure-preserving mappings between empirical structures and mathematical structures. These mappings are possible *because* of structural similarities that obtain between

such structures, and the mappings between the structures allow mathematics to represent magnitudes of empirical attributes. In many cases the existence of such mappings is taken for granted, and indeed is so familiar that the mapping may be considered to hide in plain sight. For instance, every time a thermometer is used to take a temperature, or cooking scales are used to weigh pasta, the existence of a mapping of some sort is presupposed, since such a mapping is presupposed when a scale is constructed, indeed the scale will be formed from the mathematical structure into which the empirical structure is mapped.

In effect, the existence of mappings between the empirical and the mathematical enables us to ascend to the level of the mathematical, represent magnitudes of empirical attributes or phenomena, perform mathematical operations on these mathematical representations, and then descend back down to the level of the empirical with an empirical result, enabling us to obtain certain facts about the empirical world much faster, or more reliably, than we could have done without using the mathematics. This, together with the fact there are no limits on the size of the empirical units of magnitude which mathematics can represent, is *why* mathematics is so useful. The question of why mathematics is useful in science is however not really the focus of the mapping account, but more for the philosophy of science, although the mapping account is, *qua* theory of applicability, extremely concerned with *how* it is that this application is useful. That is to say, a theory of applicability must not just account for why and how mathematics can be applied, but how it can be *usefully* applied. I could perhaps, with a suitable interpretation, apply the metaphysics of Thales of Miletus to Quantum Mechanics, but it is highly doubtful that this would be in any way useful.

In many cases the domain of mathematical structures for the representation of magnitudes of empirical attributes will be the real numbers, and the attributes measured will be scalar, but by no means is this generally the case. Indeed the mathematics used in physics frequently involves vectors, matrices, Hilbert spaces, and a great variety of other abstract mathematical objects. Because the subject matter of mathematics is all possible (i.e. self consistent) structures, if more exotic structures are required as models, as in some theoretical physics, platonistic

mathematics is in principle able to provide them. However the paradigm I shall focus upon in this chapter will be that of the real numbers as the required mathematical structure, partly because of the ubiquity of that paradigm, and partly because examples involving the real numbers are reasonably simple and sufficiently illustrative of the representational role of mathematics generally. The question of which properties an empirical structure needs to possess in order to be mapped into a chosen mathematical structure, and what extra properties it needs to possess to be measured in a certain way, is, as has been said, the subject of measurement theory.

Measurement is an important and vital part of the mathematical description of empirical phenomena, which is essential if laws are to be discovered, knowledge of them refined and their accuracy tested. As I said above, since the mapping account is about mappings between structures, and so is measurement theory, the mapping account draws heavily upon the resources of measurement theory. Indeed it would not be an exaggeration to say that measurement theory can be considered a proper part of the mapping account of applicability despite the fact the former predates the latter. I shall begin by outlining the two major approaches to measurement (2.2.2) and go into some detail about concatenation (2.2.3). I then move on to the second of these approaches, the representation-theoretic approach, and consider what sorts of mappings it is that we want to prove the existence of and what it is that we are mapping from (2.2.4). I then discuss which axioms are required to prove such a mapping exists (2.2.5). Finally I consider a philosophical issue concerning the empirical interpretation of the results of measurement (2.2.6).

2.2.1. Measurement Types, Procedures and Representation Theorems.

Measurement theory explains how a mathematical structure can measure an empirical structure, which involves, *inter alia*, showing that a mathematical structure can represent an empirical structure. Whether it can do so depends on

some of the properties of the empirical structure, that is, on the properties of the attributes (length, mass, temperature, etc.) which we wish to measure.

Measurement Types. There are different types of measurement depending on the properties of the empirical attributes in question and how they are to be related. Specifically there are three main types of measurement, at least in the natural sciences: ordinal measurement, extensive measurement and difference measurement. Showing that an empirical structure can be ordinally measured simply involves showing that an empirical domain with a comparison relation can be represented by the real numbers ordered under ' $>$ ', that for all attributes a and b in the empirical domain, $a > b$ iff $\phi(a) > \phi(b)$, where ϕ is a function taking magnitudes of empirical attributes to real numbers. Extensive measurement is more complicated, because showing that an empirical structure is extensive involves showing that the real numbers ordered under $>$ with addition can represent a domain of empirical attributes not merely ordered by a comparison relation, but also involving an operation of concatenation \circ , the connecting together of attributes with *additive*, that is, addition-like, properties. This is to say that to show an empirical structure is extensive is to show that it is ordered *and* that it is additive.⁷ A simple example of an extensive attribute is length with concatenation. Finally, to show that the attributes of an empirical structure can be measured using difference measurement involves showing that pairs of pairs of magnitudes of a given attribute (a,b) , (c,d) – an example would be a pair of pairs of temperatures – have the property that for all elements in the empirical domain, $abDcd$ iff $|\phi(a) - \phi(b)| > |\phi(c) - \phi(d)|$, where D is the has-a-greater-range-of-magnitude relation of two pairs of magnitudes of a given (differential) attribute.

In what follows I shall primarily discuss extensive measurement, as it provides many measurement examples, whereas ordinal measurement is really too basic to be very interesting here. Evidently there are significant differences between extensive and difference measurement, but there is no need to go into them in this

⁷ For an operation to be additive is for it to satisfy the property $\phi(a \circ b) = \phi(a) + \phi(b)$, about which more below.

thesis – a thorough example of extensive measurement should be sufficient to show how it is that mathematics can represent and measure magnitudes of empirical attributes. To reiterate, I have just discussed the three basic types of measurement: ordinal, extensive, and difference, and mentioned what properties attributes in the empirical domain of the empirical structure must have if they are to be measureable by one of these types of measurement. There are two main ways of *showing* that an empirical structure has such properties. The first of these ways is to use a measurement procedure for “assigning numbers to objects or events on the basis of qualitative observations of attributes” (Krantz *et al* 1971 p.2). The second, which I shall be more concerned with here, is to prove a representation theorem. No discussion of measurement is complete without an account of measurement procedures however, so I shall briefly provide this.

Measurement Procedures. There are two important measurement procedures, the latter more complex than the former insofar as the first only shows that an empirical structure can be ordinally measured, the second by contrast shows that it can be extensively measured. These procedures are the *ordinal measurement procedure*, and the procedure of *counting units*. In the examples that follow I am going to discuss assigning numbers to rods in virtue of the magnitude of the attribute of length that the rods possess. To assign numbers to rods using the ordinal measurement procedure we take some real number, it doesn’t matter what it is, and assign it to that rod. Then we take the next rod. If that rod is longer than the first rod, we assign it a larger number, otherwise a smaller. We do this until all the rods have numbers, and we won’t run out of real numbers to assign in this manner as the reals are dense – between any two there is always another one. It is clear that this method of assigning will satisfy the following restriction, that is, it will form an ordinal measure: for any two rods a and b , $a > b$ iff $\phi(a) > \phi(b)$.

The method of assigning numbers by counting units is more complex insofar as it pertains to extensive measurement, concerning both ordering and concatenating. The concatenation of rods involves laying the rods end-to-end in as straight a line as possible. We can define what is called a *standard sequence* of a unit

u , by selecting one of the rods as the unit rod u . (Krantz *et al* use the example of a metre stick, which contains a standard sequence of a thousand millimetres). Assume we have copies of rod u (u' , u'' , etc). By concatenating the copies to each other we get the following sequence: $1u = u$, $2u = u \circ u'$, $3u = 2u \circ u''$... We need to assign a number (a 'length') to each concatenation. Thus we will get $\phi(u) = 1$, $\phi(u \circ u') = 2$ and so on. Any other rod, call it b , that is not a concatenation of u and which is longer than nu but shorter than $(n+1)u$, b will be assigned a length $\phi(b)$ between the length $n\phi(u)$ of nu and the length $(n+1)\phi(u)$ of $(n+1)u$, similar to how numbers were assigned to rods in the case of ordinal measurement. This assignment guarantees that the rods and their concatenations form an ordinal measure. Additionally it is clear that the numbers assigned to the rods are additive with respect to concatenation, that they satisfy $\phi(a \circ b) = \phi(a) + \phi(b)$, since if we have to concatenate n copies of u to get a rod the same length as a and n' copies to get a concatenation the same length as b , then we must concatenate $n + n'$ copies of u to get a concatenation the same length as the concatenation of a and b . A more detailed investigation of this measurement procedure will need to explore the axioms that such an extensive empirical structure will need to satisfy in order for the measurement procedure to be carried out.

Representation Theorems. However, as I mentioned several paragraphs ago, the second approach to measurement is the approach that is more directly relevant to the mapping account of applicability, as it involves proving that there is a mapping between the empirical and mathematical structures. Rather than using a measurement procedure to allocate magnitudes of empirical attributes to numbers we can "look at the properties of the numerical assignment" (Ibid. p.9). Krantz *et al* succinctly summarise the second approach: "[f]rom this standpoint, measurement may be regarded as the construction of homomorphisms (scales) from empirical relational structures of interest into numerical relational structures that are useful" (Ibid. p.9). Krantz *et al* say more about what the second approach involves when showing that an empirical structure may be extensively measured:

...we may pose the following question: Given a set of rods, the comparison relation $>$, and concatenation \circ , what assumptions concerning $>$ and \circ are necessary and/or sufficient to construct a real-valued function ϕ that is order preserving and additive...[t]his question still asks for axiomatization [i.e. just as the procedure does]...however the conclusion aimed for is not that a certain procedure is possible, but rather that a numerical function ϕ satisfying certain properties exists. (Krantz *et al* 1971, p.8).

That is, not that it is possible to construct such a mapping, but that such a mapping exists. Let's restate what is required in the representation-theoretic approach to showing that a set of e.g. rods can have their length measured extensively: we begin with a set A of the rods, and their copies, and their (finite) concatenations, together with the comparison relation and concatenation operator, that is, we begin with the empirical relational structure $\langle A, \succeq, \circ \rangle$.⁸ We need to know what has to be the case about $\langle A, \succeq, \circ \rangle$ in order for $\langle A, \succeq, \circ \rangle$ to be mapped, with its structure preserved, into a numerical relational structure, say $\langle R, \succeq, + \rangle$, by the mapping function ϕ .

A representation theorem says for a given empirical relational structure, if it satisfies certain axioms, *there is a mapping*, which preserves structure, of the empirical structure into a given numerical relational structure. So in other words what we need to know is which axioms the empirical relational structure has to satisfy in order for the appropriate representation theorem to follow, and thus for us to prove that there is such-and-such a mapping. Different numerical relational structures can do the job of representing empirical relational structures equally well, but of course some structures may seem more 'natural' than others. That different structures can do the job is established formally by a *uniqueness theorem*, proved after the representation theorem, which states that the representation is unique up to some sort of transformation. i.e. is invariant under that transformation. Thus one mathematical structure will be transformable into another mathematical structure that will represent the empirical relational structure equally well, at least from a purely formal perspective. Unlike the mathematical structure, which so long as the uniqueness theorem holds can be any of a variety of possible structures, the

⁸ Concatenation is discussed in detail in the next subsection, 2.2.2, Until it is discussed, please allow that it is an uncontroversial operation.

empirical structure must be fixed, and obey very specific axioms, in order to be extensively (or ordinally, or differentially) measured:

What is invariant [i.e. unlike the numerical structure]...is the empirical relational structure and its empirical properties, some of which are formulated as axioms. A set of axioms leading to representation and uniqueness theorems of fundamental measurement may be regarded as a set of qualitative (that is, non-numerical) empirical laws. In some cases, as in the measurement of length, these laws are rather trivial...[i]n other empirical contexts the axioms can be quite interesting and non-obvious. (Krantz *et al*, 1971 p.13)

We must shortly turn in 2.2.4 to the issue of *which* axioms are required to prove which homomorphisms exist. This is not as straightforward as it may seem as there are many axioms to choose from, and several different types of homomorphism which can be established by such axioms. We look at this in 2.2.3. But first a word or two about concatenation.

2.2.2. Concatenation

The nature of the comparison relation is quite clear, but I feel I am overdue to say a little more about the concatenation operation \circ and some of the issues associated with it. Concatenation is represented in the mathematical relational structure, at least as far as extensive measurement is concerned, by the operation of addition. This expressed by requiring that the following condition (which we saw above also in the counting-of-units measurement procedure) holds: $\phi(a \circ b) = \phi(a) + \phi(b)$.⁹ That is to say, the concatenation operator has addition-like, or additive, properties. A minimal restriction of what counts as a concatenation, even more general than any restrictions arising out of particular measurement types, is that a concatenation operator is *a binary operator that takes members of the Cartesian product $A \times A$ of a set A to members of A* . This may be regarded as a formal restriction on what counts

⁹ You can see that this condition holds for both metres and diagonal metres. The number representing the concatenation of two diagonal metres will be the sum of the numbers representing those diagonal metres, just as in the linear case. See below for more on diagonal metres.

as concatenation. If an operation does not take members of the Cartesian product of a set A to members of A then it is not a concatenation operation. Thus, for instance, the act of holding hands with a person does not make two people into one person, so holding hands is not an operation of concatenation.

This restriction, though delimiting the set of concatenations, still permits a broad range of operations to count as concatenations. For we will not want to say that every binary operation meeting this restriction will count as a concatenation, indeed for the most part there will be one intended operation, with perhaps some other operations satisfying the formal restrictions of concatenation and yielding non-intended but formally-acceptable interpretations. For instance when talking about the concatenation of rods we usually mean placing them end to end so they form a straight line. Technically, to take two rods a and b to A , we would need to weld them end to end if they were metal, but for the sake of the discussion of measurement such specifics are largely ignored, and in this case it is clear that welding end-to-end for instance is a good example of concatenation. Thus we can see that generally speaking concatenation is a fairly idealised operation, since although (as in the welding example) there might be ways we can physically carry out a concatenation and take members of the Cartesian product of A to A , this is not normally done. Rather we tend to assume that concatenation makes sense and don't worry about the physical details. This should not cause any philosophical issues since if pressed we can attempt to find a genuine empirical operation that the concatenation operation is a placeholder for, as in the welding of metal rods case.

One possible response is that despite its generality, the Cartesian product restriction is *too* restrictive, as it does seem to result in the issues just outlined, the difficulty in finding a suitable empirical operation. On the other hand though, dropping the restriction allows too many possible candidates for concatenation, the 'holding hands' case above being an example. I think though that it is better to have a stronger conception of concatenation that may need further work to be applied on a case-by-case basis, rather than an overly weak conception that allows as concatenations operations that don't seem to even *prima facie* have the character we want. Perhaps there is another restriction that restricts concatenation

operations to all and only those operations that we want to actually refer to as concatenation, without being either too weak or too strong. The search for such a restriction would be a fascinating research project in measurement theory, in its own right, but whether successful or not the result of such a project would be a detail of only passing relevance to the mapping account of applicability.¹⁰

There are examples of operations that satisfy the Cartesian product restriction above but which are not usually intended. For instance ‘crazy plumbing’ or ‘crazy welding’, whilst resulting in a rod or tube with a definite length, and whilst taking members of the Cartesian product of A to A , is clearly not what we usually mean by ‘concatenation’. I do not know if crazy plumbing will satisfy the axioms of extensive measurement even though it does meet the Cartesian product restriction.¹¹ There is however at least one non-standard form of concatenation that does satisfy these axioms. This is Ellis’ conception of concatenation based on right-angled addition, which rather than giving the usual interpretation of concatenation as $\text{length}(a) + \text{length}(b) = \text{length}(c)$ – we might call this ‘linear’ concatenation – gives rise to instead the interpretation $\text{length}(a) + \text{length}(b) = \text{square root of } (\text{length}(a)^2 + \text{length}(b)^2)$. Karel Berka explains that “it can be geometrically shown that [this] satisfies all axioms put down for length measurement with a linear interpretation of the operation of concatenation” (Berka 1983, p.156). The units of this orthogonal concatenation are not meters but rather ‘diagonal meters’, where the length of a diagonal meter is equal to the square root of the length of a metre.

In the case of length we see there is a plenitude of potentially eligible concatenation operations, some more natural than others. Other attributes seem to have only one plausible operation of concatenation: the concatenation of electric cells so that current can flow involves circuits being physically connected, positive terminal to negative terminal, but it is not required that this connection form a straight line. Mass (or a certain range of masses at least) also possesses a clear concept of concatenation, at least for the operationalist, namely the placing of two

¹⁰ Assuming that such a project did not show that the very concept of ‘concatenation’ is incoherent! I have no more space to go into any more detail on this here however.

¹¹ Extensive measurement is discussed below at 2.2.4.

objects in the same balance pan. For other attributes we may struggle to find an operation of concatenation at all, in the case of temperature for example. It is temperature differences and not temperatures *per se* that are measured, that is, $abDcd$ iff $|\phi(a) - \phi(b)| > |\phi(c) - \phi(d)|$. Difference measurement axioms systems can have additive properties, and indeed one axiom of difference measurement is $\phi(ac) = \phi(ab) + \phi(bc)$, but as Krantz explains "...we cannot concatenate ab with cd except by mapping ab, cd onto equivalent intervals such that the lower endpoint of one interval coincides with the upper end point of the other..." (Krantz *et al*, p.147). But whatever sort of concatenation operations find application, the formal theory of measurement can accommodate them.

Once we arrive at a particular concatenation operation we also have to recognize that there is the question of what concatenations are physically possible given our conception of that operation. This is because not every concatenation capable of being represented by a mathematical structure *is* physically possible. Now it might be thought that there is a potential problem here for speaking of the results of some concatenations. The problem runs as follows: although *formally* speaking even a finite set of elements exhibiting some magnitude of the empirical attribute in question is closed under the operation of concatenation - because the object that results from concatenating two elements of the set of objects with extensively measureable attribute P is also an element of the set of objects with P - it might be *physically* impossible to concatenate some elements. Perhaps, as Krantz *et al* point out in their (1971, p.82), there may simply be no adequate space. Or amount of substance After all, we can physically concatenate one mass of hydrogen with another by say, placing them in some machine that measures mass down to the subatomic level, but we cannot iterate this concatenation beyond a certain point (say 10×10^{100} times)¹² because regardless of the size of the machine, there will come a time when, laws of physics and feasibility aside, this is more concatenation operations than there are hydrogen atoms in the universe. Moreover the opposite case might occur - we may concatenate two masses of uranium and a nuclear

¹² The operationalist overtones here are not intended to suggest any operationalism in this thesis, the operationalism here is for example only.

reaction occur, with much of the mass being converted into energy. In this case it would seem that $\phi(a \circ b) \neq \phi(a) + \phi(b)$ despite the fact that mass is an extensive magnitude. There are, as I see it, two responses to this problem.

The first is to design more restrictive axioms that can be added to the axioms of extensive measurement and which are sensitive to the context of measurement, restricting the operation of concatenation. This response however would mean extra axioms would need to be provided for every context of measurement, and depend on the attribute in question. Whilst this is acceptable, in the sense it would not prevent the representation theorem being proved (assuming the new axioms don't affect the consequences of the older axioms) it is somewhat against the spirit of the mapping account. The mapping account *qua* theory of the applicability of mathematics to scientific theories appreciates that the axiom systems for measuring particular extensive attributes will likely need to be supplemented by additional axioms depending on what attribute is being measured, and indeed to deny this would make the mapping account insensitive to specific theories, and deprive it of much of its explanatory value, but to add axioms even for the specific context of measurement as well as the specific attribute would make the mapping account extremely unwieldy and unworkable as a general theory of applicability. It is an open question whether the authors of *Foundations of Measurement* intend to go this far when they promise the reader that further into that work they will “develop an alternative theory that embodies these limitations and provides a more realistic account of length and mass measurement” (Krantz *et al.* p.82), but I think it doubtful.

The second, and superior, response is to follow Pincock (2004b, p.148) and drop the operationalism that seems to be the methodology for many measurement theorists, and allow the result of anything that can in principle be concatenated without paying attention to spatial or temporal or material considerations. Can the mapping-account theorist get away with this? It seems so, since he can say counterfactually ‘such and such a concatenation is impossible, but if it were possible, the concatenated property would have magnitude x ’. This is compatible with the idea that sets of axioms should be tailored towards the specific empirical attributes

which are being measured. Thus although there are actual physical concatenations, we can recognize that not all concatenations permitted by the formal theory of measurement are physically possible, and there will of course be some sorts of concatenations we currently have no reason to think should not be physically possible which are impossible, just as in the past there have been concatenations that have not been thought impossible but turned out to be so. Let us define the concatenation of two gas giants as the merging of those gas giants. Before nuclear fusion was understood, there would have been no reason to think that the mass of the result of concatenating 75 Jupiters in the above sense would not be the same as the sum of the mass of each Jupiter. But at the same time, if we wish, so long as we are clear, we can say “if it wasn’t for fusion, the mass of 75 merged Jupiters would be x ”. This does not affect the mapping account, as whilst accepting that not every concatenation will be possible, it would be unreasonable to expect the formal theory of measurement to tell us *a priori* which concatenation operations will or will not find application or are or are not possible, and we would not expect a formal theory of measurement, or indeed a philosophy of applied mathematics, to give us this information. We accept that physically possible concatenations are only a subset of formally acceptable concatenations, and we can talk about idealised, or counterfactual, cases of concatenation when we need to.

2.2.3. What Needs to Be Mapped? Choosing a Domain and a Mapping.

The selection of structure-preserving mapping, or homomorphism, will be guided by the empirical structure we are mapping from. Types of homomorphism include *monomorphism*, an injective or ‘into’ mapping, *epimorphism*, a surjective, or ‘onto’, mapping, and *isomorphism*, a bijective, or 1:1 mapping (note this does not exhaust the category of homomorphisms but is sufficient for our discussion). In cases where the mapping is an isomorphism the inverse of the isomorphism will easily take us from the mathematical structure back to the empirical structure, but if the mapping is not an isomorphism then two separate mappings, one from the empirical to the

mathematical, and one from the mathematical back to the empirical, will be required, because of the structural differences between the empirical and mathematical structures. For instance, suppose we want to map the masses of some chemical compounds into the real numbers. Given that some compounds have the same mass, in this instance more than one compound will be mapped to a real number, a many-one mapping, or epimorphism. This mapping that would be required to handle this would be complicated and the situation messy. Fortunately there is a natural way to avoid this, using equivalence classes. This method is to prove an *isomorphism* between a disjoint set **A** of equivalence classes of the empirical objects forming domain A, and the real numbers.

An equivalence class is a set of objects which are related by an equivalence relation, where an equivalence relation is any relation that is reflexive (aRa), symmetric (if aRb then bRa) and transitive (if aRb and bRc then aRc). An example of such a relation is 'being the same length', or 'having the same mass'. Using equivalence classes means we avoid the many-one complexity of there being more than one compound a with the same mass, for instance, by mapping the equivalence class **a** of objects that share (as far as experiment can determine) the same mass, to a real number. Thus there will only be one real number associated with each actual mass. Moreover there will be no need for a new mapping every time a compound is added to one of the classes since sets and classes are determined only by their members and repetitions in membership don't lead to set-theoretic differences.

However unless we hold the view that a unique equivalence class containing actual compounds corresponds to every real number (a highly dubious empirical claim) this will still only give us a partial isomorphism, since although each class will correspond to some unique real number, some real numbers will not correspond to any class of actual objects sharing the mass, since there may be no such objects existing in the universe in order to form such a class.¹³ One possible issue here is

¹³ The Bueno and Colyvan paper gives a clear account of partial isomorphism in terms of partial relations which I will paraphrase here. First we define a *partial relation* R – this is relation which is not defined for all members of the domain D . Formally R is an ordered triple of disjoint sets $R_1 R_2 R_3$ which together make up the domain. R_1 consists of the n -tuples elements of the domain satisfying R , R_2 consists of the n -tuples which do not satisfy R , and R_3 consists of the n -tuples for which it is not defined whether they satisfy R or

that there will be a different mapping coming into and out of existence every time a compound with a new unique mass, and *a fortiori* a new equivalence class, is added to the empirical domain. For every a time a compound with a new mass came into existence it would engender a new equivalence class that had not been mapped previously to a real number, and every time the only compound with a particular mass went out of existence, the equivalence class whose only member it was would be destroyed, since the existence of an equivalence class depends on the existence of its members.

This might be thought to pose the problem that a new partial mapping will need to be proved every time the domain of equivalence classes changes, and so the mapping account theorist would, in order to explain applicability, need to prove a representation theorem for each case, something that could be avoided if there was a full isomorphism. This is however untrue, since it is provable that an empirical structure satisfies the axioms of extensive measurement iff there is some representation theorem showing the existence of a structure-preserving mapping from that structure to the real numbers (Krantz *et al* p.74 – Theorem 1). Thus regardless of the changes to the empirical domain, so long as the changes to the domain don't affect whether or not the structure satisfies the axioms of extensive measurement there will still exist such a mapping, without it having to be manually reproven. The next subsection (2.2.4) discusses the axioms of extensive measurement in more detail. If the mapping account theorist is happy to accept partial isomorphisms that is absolutely fine. If he does not like the idea of partial mappings, perhaps for some ideological reason, and wants a full isomorphism, then there is another approach, at the cost of introducing possibilia, which the mapping account theorist may have independent reasons for wishing to avoid.

not. A *partial structure* is an ordered pair of a domain D and a partial relation. A *partial isomorphism* is a function ϕ from one partial structure $\langle D, R \rangle$ to another $\langle D', R' \rangle$ such that (1) ϕ is bijective and (2) for all members x and y of D , xR_1y iff $\phi(x)R'_1\phi(y)$ and xR_2y iff $\phi(x)R'_2\phi(y)$. If R_3 is empty we have a standard isomorphism, if it nonempty then this means some components of D that are in R_3 are not actually mapped by ϕ . (Bueno and Colyvan 2011, pp. 358-359). Put simply, a partial isomorphism is an isomorphism where not all the elements of a domain are mapped into another domain. If we define the domain D as all possible values of some empirical attribute then we can stipulate that R_3 contains those values that have not been actualised – so only the values that have resulted from some concrete measuring act, for example, are actually mapped into the co-domain D' , which for this example could be the real numbers.

We can get a full isomorphism in the chemical compound/mass case if we allow equivalence classes of not just actual, but actual and logically possible, compounds. So we want a mapping between equivalence classes of actual and possible compounds with the same mass, and the real numbers. Assuming that mass is a property that is continuous like the reals, there will be an uncountable amount of such equivalence classes, and thus it will be possible to prove a full isomorphism between them.¹⁴ *In the rest of this chapter however nothing much rides on whether the partial isomorphism approach, or full isomorphism plus modal apparatus approach, is selected, apart from where indicated, and my language will be neutral.*

Since both approaches utilise equivalence classes there is one issue with these classes that I want to address, namely the issue of how you concatenate a class. It sounds artificial to say classes can be concatenated. But this is easily got around – Krantz *et al* explain that we can interpret the concatenation **a o b** of two equivalence classes **a** and **b**, as being equivalence class **c** formed by the concatenation $a \circ b$ of an object a and an object b from each class. (Ibid. p.87). We are thus really talking about the concatenation of empirical objects rather than the concatenation of classes, though it is convenient to refer to the concatenation of classes nevertheless. The concatenation of an actual compound a in **a** with another actual compound b in **b** will result in an object c that will then become a member of a different equivalence class, **c**. This may present a problem for the full isomorphism + possibilia theorist however since we cannot talk about the concatenation of an actual compound and a possible compound (or indeed any possible object) as concatenation is intended, at least in principle, as an actual empirical operation. It thus only makes sense to say that actual objects and not possible objects can be concatenated, though we can speak counterfactually about what the resulting value of some attribute of two possible objects would be if they were in fact actual and concatenated. This restriction is easily observed and so causes no difficulties.

¹⁴ If it turns out there is some such thing as the smallest possible mass, or equivalently, smallest possible unit of energy, the philosopher will be thrown back on partial isomorphisms, perhaps the wiser option.

2.2.4. Selecting the Axioms for Extensive Measurement.

So we know what the structures involved in the mapping will be, and what sort of mapping we want. Now we need to know what axioms the empirical structure needs to satisfy in order that the representation theorem for extensive measurement can be proven, that is, that $\langle \mathbf{A}, \succ, \mathbf{o} \rangle$ can be represented by $\langle \mathbf{R}, \geq, + \rangle$, or in other words that an isomorphism function ϕ taking members of \mathbf{A} to members of \mathbf{R} exists. The Representation Theorem for Extensive Measurement is as follows:

Representation Theorem for Extensive Measurement

There is a real-valued function ϕ on \mathbf{A} from the empirical relational structure $\langle \mathbf{A}, \succ, \mathbf{o} \rangle$ to the numerical structure $\langle \mathbf{R}, \geq, + \rangle$ such that

- (i) for all $\mathbf{a}, \mathbf{b} \in \mathbf{A}$, $\mathbf{a} \succ \mathbf{b}$ iff $\phi(\mathbf{a}) > \phi(\mathbf{b})$, and
- (ii) for all $\mathbf{a}, \mathbf{b} \in \mathbf{A}$, $\phi(\mathbf{a} \mathbf{o} \mathbf{b}) = \phi(\mathbf{a}) + \phi(\mathbf{b})$.

if and only if

$\langle \mathbf{A}, \succ, \mathbf{o} \rangle$ is an extensive structure (That is, if and only if $\langle \mathbf{A}, \succ, \mathbf{o} \rangle$ satisfies the axioms of extensive measurement).

This real-valued function is the mapping function in question. If we want to get a representation theorem for e.g. the mass of chemical compounds we can stipulate that \mathbf{A} is the set of equivalence classes \mathbf{a}, \mathbf{b} of chemical compounds a, b of \mathbf{A} the set of chemical compounds, and the equivalence relation on the members of the classes is 'having the same mass', i.e. ' $a \sim_m b$ '; \succ is the comparison relation ' x has a greater mass than y ' between equivalence classes (which depends on the empirical comparison relation \succ between their members a and b); and \mathbf{o} is the concatenation of the equivalence classes) standing for the concatenation \circ of a and b , that is, the mass of the two compounds being considered together.

The proof proceeds by deriving the existence of such a function from the axioms for extensive measurement which are given below, and by deriving the axioms of extensive measurement from the existence of such a function. The proof is generally given by reducing the above theorem to its analogue for strictly ordered groups, that is, Holder's Theorem. (Krantz *et al* 1971, p.75). I do not repeat this proof here, as it has been discussed (*ad nauseam*) in the literature, and so merely adds needless technicality to this thesis. However if the reader is in any doubt as to the sufficiency of the axioms of extensive measurement to prove the existence of the function, I refer them to Krantz *et al*, p.81. As Fred Roberts explains, "a direct proof of the sufficiency of the conditions can be found in Krantz *et al*. [1971, Theorem 3.1]" (Roberts 1979, p.128). Other versions of the proof can be located in Roberts and Luce (1968) and Holman (1969). The interested reader may also consult Krantz *et al* 1971, pp.15-16 for the corresponding proof for the representation theorem for ordinal measurement. I shall look now at whether the axioms are necessary, since this relates to the interesting issue of which axioms the empirical structure needs to satisfy if it is to both be extensively measured and not be made subject to axioms stronger than those actually required.

Sets of axioms that are *sufficient* for extensive measurement have been known for a very long time, the first axiom set being given by the mathematician Otto Holder in 1901 for mass measurement, based on even earlier work by Hermann von Helmholtz. Holder provided four axioms for extensive measurement (although the first is really three axioms in one):¹⁵

- (1h) $\langle A, \circ \rangle$ is a group (i.e. it is associative, with an identity element and inverse)
- (2h) $\langle A, > \rangle$ is a strict simple order
- (3h) Monotonicity: $(\forall a)(\forall b)((a > b) \text{ iff } (\forall c)((a \circ c) > (b \circ c)) \text{ iff } ((c \circ a) > (c \circ b)))$
- (4h) Archimedean Axiom: For all a, b in A , if $a > e$ where e is the identity for $\langle A, \circ \rangle$, then there is $(\exists n)((n \in \mathbb{Z}^+) \ \& \ (na > b))$.

¹⁵ The basis for much of the following few paragraphs is Roberts (1979) pp.122-129.

It is well known in the literature (Roberts 1979, p.127) that although Holder's axioms were sufficient for proving the representation theorem for the extensive measurement of mass, but they were not all necessary, indeed three of them can be weakened considerably and extensive measurement still hold. For instance, it is not necessary that $\langle A, \circ \rangle$ is a group, or even that \circ be associative, in fact it need only be weakly associative. Furthermore the order condition does not need to be as strong as a strict simple order, a weak order (a transitive and connected order) will suffice.¹⁶ Finally, Holder's formulation of the Archimedean axiom must be altered since it relies on the assumption that there is an identity element, i.e. that $\langle A, \circ \rangle$ is a group, and we have already said that is not a necessary assumption. Roberts, and Hand (2004, p.35), provide the following alternative axiom set, which is both necessary and sufficient for extensive measurement:¹⁷

1. Concatenation is weakly associative: $(\forall a)(\forall b)(\forall c)((a \circ (b \circ c)) \sim ((a \circ b) \circ c))$
2. $\langle A, > \rangle$ is a weak order: (empirical relation $>$ is transitive and connected)
3. Monotonicity: $(\forall a)(\forall b)((a > b) \text{ iff } (\forall c)((a \circ c) > (b \circ c)) \text{ iff } ((c \circ a) > (c \circ b)))$
4. Archimedean axiom: $(\forall a, \forall b, \forall c, \forall d)((a > b) \supset (\exists n)((n \in \mathbb{Z}^+) \ \& \ (na \circ c) > (nb \circ d)))$

I shall begin with a consideration of axiom 4. We know that some sort of Archimedean axiom is required, since it is this axiom that says that we can always compare magnitudes of attributes of empirical objects, and magnitudes of attributes of concatenations of empirical objects, which is something we definitely want. The fact the positive real numbers satisfy a version of the Archimedean axiom is what makes them so useful for *comparing* the magnitudes of the attributes that they represent, as well as merely representing them. We know that for any positive real number x which is less than a real number y , x can be made greater than y by

¹⁶ This is explored in more detail in three pages time.

¹⁷ This will deliver an isomorphism only if no a and b are mapped to the same real number. To prove our representation theorem and get an isomorphism we would replace A , a , b and c with the set and equivalence classes **A**, **a**, **b**, **c**, and discuss the comparison and concatenation of such classes rather than the empirical objects. However I have already explained how these equivalence classes can be interpreted as having extensive properties, arising directly from the empirical properties of their members, so the theorem would follow *mutatis mutandis*, by replacing **A** with A and so on.

multiplying it n times, where n is a positive integer, that is, that $(\forall x, \forall y)((x, y \in \mathbb{R}^+) \& (y > x)) \supset (\exists n)((n \in \mathbb{Z}^+) \& (nx > y))$. This is just part of what it is to be a positive real number, and means that we can compare all positive real numbers. The question is, does the empirical structure which the positive reals are representing also have Archimedean properties, i.e. satisfy some empirical version of the Archimedean axiom? The first thing is to ascertain what na means if a is some empirical attribute of a given magnitude. This is easy to specify, inductively, given concatenation:

$$\begin{aligned} 1a &= a \\ 2a &= 1a \circ a \\ &\dots \\ (n+1)a &= na \circ a \end{aligned}$$

And we know, since concatenation is (weakly associative) that $((a \circ a) \circ a)$ is the same as $(a \circ (a \circ a))$, i.e. that we can drop the brackets and just say $a \circ a \circ a$. In practice, when measuring an attribute we choose one exemplar a of that attribute, such as the standard metre in Paris, to be the standard unit of that attribute. This choice is fairly arbitrary but will in practice be guided by considerations of ease of use, ability to convey what is meant by the unit, and so on. This is why in the early history of measurement, before the development of sophisticated measuring devices, many units were based on human body magnitudes (feet, hand-spans etc). After the unit is chosen, questions such as how long an object is will usually be answered (in the natural sciences at least, though not in everyday usage) by saying how many units it is, rather than how it relates to other (perhaps nearby) empirical objects with the attribute (and trivially, which are not copies of the exemplar in question).

Given that we can give an inductive definition of na using concatenation, is that enough to say that an empirical structure with concatenation has the Archimedean property? It will do if the empirical domain A is sufficiently rich, as it would be if we adopted the possible equivalence classes approach. If we go down the partial isomorphism road it may not be, but I must refer the reader to Krantz *et*

al 1971 for more on this topic, I have no space to pursue it here. The inductive definition of na does not however tell us the specific form the Archimedean axiom describing the Archimedean property will need to have. We know that Holder's version of the axiom was sufficient, but not necessary, since there was no need to assume that $\langle A, \circ \rangle$ was a group. This leaves open the possibility that Holder's axiom, minus the part of it that discusses groups, is the necessary axiom, i.e. $(\forall a, \forall b)(\exists n)(na \succ b)$. However, the substitution of this axiom yields an axiom set that is necessary but not sufficient for extensive measurement, the converse of the Holder case, since the axiom set is too weak to yield all and only extensive structures. In order for the axiom set to be necessary *and* sufficient the Archimedean axiom needs to be strengthened. The stronger form is $(\forall a, \forall b, \forall c, \forall d)((a \succ b) \supset (\exists n)((n \in \mathbb{Z}^+) \ \& \ (na \circ c) \succ (nb \circ d)))$. Mathematicians are very concerned with which axioms are essential for proving which theorems for there are a myriad of sufficient axioms systems but only one axiom system which is necessary *and* sufficient for proving a given theorem or set of theorems, the theorem in this instance being the representation theorem for extensive measurement. Roberts explains why this stronger formulation of the axiom is necessary:

To see that Axiom [4] is indeed an Archimedean axiom (that is, that it reflects the Archimedean properties of the reals) and that it is necessary, let us observe that $(a \succ b)$ implies $\phi(a) > \phi(b)$, so $\phi(a) - \phi(b) > 0$. Thus, by the Archimedean property for the reals, there is a positive integer n such that $n[\phi(a) - \phi(b)] > \phi(d) - \phi(c)$, that is $n\phi(a) + \phi(c) > n\phi(b) + \phi(d)$.¹⁸ From this, since ϕ is additive, it follows that $\phi(na \circ c) > \phi(nb \circ d)$, or [by the fact that for ordinal measurement that for all $a, b \in A$, $a \succ b$ iff $\phi(a) > \phi(b)$] $(na \circ c) \succ (nb \circ d)$. (Roberts 1979, p.128, I have replaced Roberts' symbols with those used in this thesis for clarity).

Thus the stronger formulation of the axiom, as well as conveying that all physical magnitudes of an extensive attribute are comparable, also explicitly accommodates a fact that follows from the Archimedean property of the reals, namely that $n[\phi(a) - \phi(b)] > \phi(d) - \phi(c)$, which the intuitive formulation did not. I trust this settles

¹⁸ If this is not obvious, try interpreting $\phi(a) = 3$, $\phi(b) = 2$, $\phi(c) = 4$, $\phi(d) = 6$ (since the differences have to be positive) and $\phi(d) - \phi(c) > \phi(a) - \phi(b)$, and $n = 3$.

the issue of why the unusual form of the Archimedean axiom is necessary in the axiom system for extensive measurement.

Hitherto I have neglected to discuss the first three axioms and I shall now remedy this. As regards axiom 1, the weak order axiom, evidently *some* sort of order constraint must be satisfied if the structure is to be measured, since order is a precondition of measurement. But why a weak order rather than another sort of order such a strict simple order? Because a weak order is sufficient for ordinal measurement, and extensive measurement is really ordinal measurement plus additivity. Since considerations about concatenation have no bearing on the order generated by $>$ there is no reason to think that anything stronger than a weak order is required. I realise I have so far said nothing to explain what a weak or simple order is, so perhaps a small digression is required.

There are many ways in which the elements of an empirical structure can be ordered. Two of the orderings most commonly found in measurement theory are weak orders and simple orders. Weak orders are those where the ordering relation R is transitive¹⁹ and connected²⁰, for example a collection of rods of any length all ordered under the 'equal-to or longer-than relation' or a collection of rods of different sizes ordered under the 'longer-than' relation to each other.²¹ It is evident that the 'equal-to or longer-than' relation is transitive, and that any rod is equal-to or longer-than itself insofar as it is equal to itself, that is, the relation is also reflexive. Weak orders are the basis for other types of stronger order, e.g. the simple order (also called a total order) is a weak order that is also asymmetric (the ordering relation R is transitive, asymmetric²² and connected), an example of which is the second case involving rods above, which clearly preserves the asymmetry requirement of the simple order, since necessarily if a is longer than b then b is not longer than a . We thus see that these order concepts really are quite easy to grasp

¹⁹ Transitivity: R is transitive iff aRb and bRc imply aRc

²⁰ Connectedness: R is connected iff for all a, b , aRb or bRa

²¹ In this example if the rods were of the same length connectedness would be violated since there would at least two rods not standing in the 'longer-than relation'.

²² Asymmetry: R is asymmetric iff aRb and not bRa

and that examples of them are very easy to find, since any attribute that can be extensively measured could be substituted for length in the examples above.

Next we turn to the weak associativity axiom and the monotonicity axiom for concatenation. I shall consider these together as both are essential for guaranteeing the additivity property of concatenation, since two crucial properties of addition are that it is associative and monotonic. It is by no means uncontentious that *concatenation* is associative however, since the associativity of additions of real numbers involves identity whereas all we can say about concatenations of empirical objects is that they have the same degree of attribute as some other concatenation up to our ability to discriminate this sameness. Thus we cannot say $(\forall a)(\forall b)(\forall c)((a \circ (b \circ c)) = ((a \circ b) \circ c))$ but only $(\forall a)(\forall b)(\forall c)((a \circ (b \circ c)) \sim ((a \circ b) \circ c))$. This latter is weak associativity, but it still gets the job done, as we are only concerned about the additivity of concatenation insofar as we are able to discriminate it, or insofar as the degree to we need to discriminate it is concerned. Monotonicity of concatenation is straightforward however, since it is evident that if a is, say, equal to or longer than b , then concatenating an object c first to a and then to b will entail that $(a \circ c) > (b \circ c)$, since the act of concatenation does not change the length of rod a or b , and the length of rods a , b and c are (we assume) constant. Moreover it is evident that the concatenation of length is commutative, that a rod a concatenated with a rod b has the same length as a rod b concatenated with a rod a , and so $((a \circ c) > (b \circ c))$ iff $((c \circ a) > (c \circ b))$. Thus monotonicity of concatenation holds.

I submit then, that the axiom system 1-4 is necessary and sufficient for the extensive measurement of extensive attributes e.g. the lengths of rods, the resistances of resistors in series or the masses of chemical compounds. We have seen how a standard unit of an attribute may be easily defined, and how different magnitudes of extensive attributes may be compared, both to each other and to a

standard unit. In essence then, we have seen how mathematics enables us to measure attributes, at least for the case of extensive measurement.²³

2.2.5. Concluding the Mapping Account.

I have given only one example of measurement in any detail, an example of extensive measurement. Measurement theory is very complex, and deals with all sorts of other structures and procedures including not only the aforementioned difference measurement, but also probability representations and the like, and involves proving representation theorems and finding necessary and sufficient sets of axioms for empirical structures containing objects with attributes much less straightforward than extensive attributes, such as the attributes measured by the social sciences. To go into such detail here is beyond my expertise, though more importantly surplus to the gist of the philosophical theory at issue, the mapping account of applicability. Naturally it is very important that measurement theory be developed to cover all the different sorts of applications of measurement that there are, but I believe we can leave that in the capable hands of the mathematicians. As the three volumes of *Foundations of Measurement* and the extensive literature show, much work has already been done.

I do want briefly to address one final issue: we have shown that mathematical structures can represent empirical structures, and that operations on mathematical structures can model operations on empirical structures, but it might be wondered 'how do we know that the results we get at the mathematical level can be translated back into accurate results at the physical level?'. This is essential to the

²³ The claim may be made that there are many other sorts of things that are measurable, e.g. twisting roads and snakes. But these are measured indirectly and reduce down to simpler or more fundamental forms of measurement. To measure a snake or a wheel on a meter-wheel we may take a piece of string, bend it round the shape, mark where it meets the snake's tail or where the end of the string meets the beginning of the wheel, and the extensively measure the string. In the twisting road case the measurer will count clicks. The first click is one meter, the second the concatenation of the first metre with a second metre, and so on, as characterised inductively above. So these seeming difficulties do reduce to extensive measurement. This is possible because the radius of the wheel is sufficiently invariant, so continued use, up to a point, will not affect the measurement.

mapping account of applicability, since we want to say not just that mathematical structures can measure empirical structures, but that operations on mathematical structures can tell us things about empirical structures that we would not have known (or at least that it would have been less easy to have known – see chapters three and four) without the mathematics.

What I specifically want to consider here is that given that we know that mathematical structures can represent empirical structures in such a way that we can use mathematical structures to measure the empirical structures, how do we know that our measurements will be accurate? For instance we know from the representation theorem for extensive measurement that if rod a is measured as $\phi(a)$ and rod b is measured as $\phi(b)$ then the measure $\phi(a \circ b)$ of the concatenation of a and b will be $\phi(a) + \phi(b)$. But how do we *know* this is actually the length of the concatenation, without measuring the concatenation directly? And even if the measurement does agree with the data, we want to know with certainty that this result will hold even for cases which we do not check. If we could not guarantee this, then measurement theory (and *a fortiori* the mapping account) would be little more than an intellectual exercise with little practical value. Fortunately this is very easy to explain. I have suggested we use equivalence classes of bearers of extensive attributes, which will ensure the mapping from the set of such classes to [some interval] of the set of positive real numbers will be an (at least partial) isomorphism. As such to get from the mathematical back to the empirical we need only take the inverse of the isomorphism, we don't need to prove the existence of any other mappings. Let me illustrate this:

We have rods a and b .

a and b are members of the equivalence classes **a** and **b** respectively.

Assuming part (i) of our representation theorem is proved, we have a homomorphism ϕ taking **a** to $\phi(\mathbf{a})$ and **b** to $\phi(\mathbf{b})$

We concatenate a and b , giving $a \circ b$, that is, **a o b**.

Again, using part (i) of the representation theorem we take **a o b** to $\phi(\mathbf{a o b})$

By part (ii) of the representation theorem, this is $\phi(\mathbf{a}) + \phi(\mathbf{b})$. Let us call the number that is the result of the addition $\phi(\mathbf{c})$.

Equivalence class \mathbf{c} contains the object c resulting from the concatenation of $a \circ b$.

But is $\phi(\mathbf{c})$ the length of c ?

The answer is that it has to be, since the inverse of ϕ is an isomorphism ϕ^{-1} from $\phi(\mathbf{c})$ to \mathbf{c} , that is, $\mathbf{a} \circ \mathbf{b}$, and since c is a member of equivalence class \mathbf{c} , $\phi(\mathbf{c})$ will be $\phi(c)$, the length of c .

I trust this settles the issue. We have thus seen how it is that measurement theory explains, among other things, what properties an empirical structure has to possess in order to be represented accurately, and measured, by a mathematical structure. Since the mapping account is concerned with explaining applicability in terms of mathematical *representations* of empirical structures, we see that measurement theory (though a branch of mathematics in its own right) can be viewed as a proper part of the mapping account. It is an essential part of the mapping account as it shows which representations are possible and how, and also shows how one class of mathematical descriptions of empirical phenomena works, viz. measurements. The mapping account can explain more than just how mathematics can be used in the measurement of magnitudes of empirical attributes however, as I shall shortly show in section 2.3, where I consider how mathematical structures can also represent relations between empirical attributes, specifically, natural laws.

2.3 Mathematics in Natural Science

In this section I look at whether the mapping account's claim that the role of mathematics in science is a representational one, that mathematics is applicable to natural science because mathematical objects and operations can represent magnitudes of empirical attributes and their relations, suffices to show how mathematics is so useful in the description and prediction of physical phenomena.

The role of natural scientists is to measure empirical phenomena and to discover laws concerning the relations of these phenomena. One application of such laws is the making of predictions, though these are more important in the applied than the theoretical sciences (engineering, astronomy and the like). Predictions do have a very important role in theoretical science though, as when a prediction is made, if it is borne out, the best explanation of this fact may be that the law is correct. At least, correct predictions serve to confirm the truth of laws, even though no empirical law can receive total verification.

There may be more to natural science than the measuring of attributes and the discovery of laws, but these are very central to the scientific enterprise, and it would be difficult for anyone to deny this, even granting Cicero's quip that philosophers occasionally like to say silly things. Fortunately analytical philosophy of science is on somewhat firmer grounds than peripateticism. In this section I shall indulge a brief digression on the subject of predictions, and then look at the measurement of attributes and what are called initial conditions (2.3.1). I will then get down to the business of the role of mathematics in the description of empirical laws (2.3.2), which will be fleshed out by a speculative section (2.3.3) that will illustrate the role of mathematics in Newton's law of universal gravitation. I conclude that the role of mathematics in natural science is transparent, and that no use of mathematics has been uncovered in this chapter that the mapping account is not fully able to deal with. The subject of whether there are examples of applied mathematics to which the mapping account does not do justice is answered in the negative in chapter five. But first a quick discussion about prediction, and the clarification of some terminology.

Natural laws not only describe how various empirical attributes are related, but once they are given a mathematical form in which such attributes' magnitudes are represented by mathematical objects they tell us very precisely what the magnitude of an attribute will be if the magnitudes of certain other attributes are known and related in the way the laws says. The magnitudes of these prior attributes are called the *initial conditions*, and the magnitude that the law tells us a certain attribute will have, if the initial conditions which are brought under the law

are what we think, or suppose, they are, is the prediction that the law enables us to make. I just said that much concern with predictions involves the applied natural sciences.²⁴ Part of the reason such sciences exist is to make accurate empirical predictions, to ensure the safety of diverse artefacts, and the success of various endeavours. For instance, if we couldn't predict how much fuel an airplane needs to reach its destination it may never make it, and if we couldn't predict the weather we wouldn't know whether to take an umbrella or a sunhat on a given day.

These all are judgements arising from the application of science, the making of predictions. And as I mentioned briefly above, predictions are also useful for theoretical science in their own right – they are one way that scientists can test whether the initial conditions and the laws they are using are complete or correct, by comparing what they allow them to predict with what actually happens. If a scientist knows with a significant degree of certainty that the initial conditions are accurately determined but the prediction does not in fact obtain then he knows that there must be some issue with his putative law or other laws upon which it depends. If his lawlike statement, his putative law, might be wrong, that what he thought was a law may not in fact be a law, or at least not the whole story about a law. But before laws can be accurately described, or indeed in many cases even approximately guessed, a scientist needs decent information about the magnitudes of the empirical attributes that a law relates, the initial conditions. The mapping account of applicability is vital to explaining how mathematics is useful in this regard. This is the subject of the following subsection.

²⁴ Note there is no such thing as a totally pure science – the more sophisticated the measurements needed for the theoretical science to be verified, the more advanced the technology will need to be, and this technology will of course rely on applied science. But by this distinction I really mean to separate the purposes of these sciences, one the business of measuring attributes, discovering laws, and making predictions, the other the business of applying these laws and making further predictions. Of course this is an oversimplified demarcation but it is useful for illustrative purposes.

2.3.1 Initial Conditions, Empirical Units and Fundamental and Derived Attributes.

The identification and description of empirical attributes is necessarily the beginning of science, since attributes are what are related by scientific laws, the discovery of which is the business of science, and the more exact the law the more important it is to be clear about what the magnitude of a given attribute is. Many experiments have one of two aims, to enable the measurement of some empirical attribute, or to permit a natural law to be observed 'in action'. In order for the experiment to proceed at all, the magnitudes of the relevant empirical attributes involved in the experiment must be known. There is no need to rehash here how and why mathematical structures can represent magnitudes of empirical attributes, that is, how and why measurement is possible, for this was shown in some detail in the preceding section, 2.2. The example there considered was that of the measurement of an extensive attribute, mass, and illustrated the need for a unit of measurement and an operation of concatenation, if anything more useful than ordinal measurement is to be possible. The unit of measurement of some attribute is what the magnitude of that attribute is described in terms of. Historically a great variety of units of attributes have been used in science, with different units being known by multiple names, and the same attribute being measured in terms of different units. Naturally this would be quite confusing, and in order to facilitate the sharing of results and scientific cooperation it is evident that some unified system of units would be required. Just such a system was adopted by the scientific community in 1960, the *système international d'unités*.

Over one hundred units are recognised in the SI system, with each unit denoting a (fundamental or derived) attribute. All the attributes (and *a fortiori* their units) were defined in terms of 'fundamental' attributes (and the units of those fundamental attributes). Fundamental attributes are those which are not described in terms of other attributes. The SI system recognised seven attributes as fundamental: charge, temperature, length, time, amount of substance, angle, and mass, with the corresponding units of Coulomb (C), Kelvin (K), metre (m), second

(s), mole (mol), radian (rad) and kilogram (kg). Although “...it is surely wrong to think that there is only one fundamental system of properties adequate to lead to numerical measurement” (Krantz *et al* 1971 p.1), and the choice of unit is a semi-arbitrary²⁵ matter of convention, some attributes need to be recognized as fundamental in order to prevent circularity, the defining of all attributes in terms of each other. Most attributes *are* defined in terms of relations of other attributes and are therefore not fundamental, but eventually this definition will bottom out at the level of the fundamental attributes. For instance force is defined in terms of mass (a fundamental attribute) and acceleration (itself a derived attribute of the fundamental attributes of length and time). So we see that force can be defined in terms of a relation of mass, length and time, and the *unit* of force is therefore defined in terms of the *units* of mass, length and time: kgms^{-2} , known for convenience as a Newton.

Whatever the level of complexity of an attribute, that is, to whatever degree it is a derived attribute, it will be the case that if the attribute is derived it is defined in terms of some empirical relation of other attributes, which will be represented by a mathematical relation or operation the relata of which, mathematical objects, represent the magnitudes of various attributes. So for instance the attribute of force is a derived attribute given by $F = ma$. This says that force is the product of mass and acceleration. Once mass and acceleration have been measured (in the case of acceleration this will often depend on prior extensive measurements of length and time, mass is of course extensively measurable), the product of the numbers that are the results of measurement represents the magnitude of the attribute of force. Note that product here *represents* an empirical relationship, it is not itself an empirical relationship. The empirical relationship is, to use Karel Berka’s terminology (Berka 1983 p.59), a functional one, a relationship of ‘empirical proportionality’ if you will.

The relationship in question is that between the component attributes of a derived attribute, and is the relationship of (assuming for the sake of the example that the derived magnitude is derived from only two components) one of the

²⁵ In principle completely arbitrary, but in practice units chosen reflect the needs and faculties of practitioners, though of course many such systems will satisfy these pragmatic constraints.

component attributes needing to be halved if the other doubles in magnitude, if the magnitude of the derived attribute is to remain the same. As such $F = ma$ as stated really means $\phi(F) = \phi(m)\phi(a)$, and if we want to convey a purely empirical relationship by $F = ma$ we would be better to say $F =_e mRa$ where R means ‘ m is empirically proportional to a ’. Since $F =_e mRa$ iff $\phi(F) = \phi(m)\phi(a)$, product, or multiplication, can represent this empirical relationship of (empirical) proportionality.

Of course, product also occurs in *impure* products of empirical magnitudes and numbers. In this instance however what is really going on is repeated addition of some unit, or fraction of that unit, and so the use of multiplication is a shorthand for iterated concatenation of units and fractions of units, rather than generally representative of a distinct relation: to say ‘the length of a is three times the length of b ’ is simply to say that if b were concatenated to copies of itself two times (in the sense that there is a copy b' of b , another copy b'' of b , the copies are not numerically identical, and the copies are concatenated with each other) and a and $b \circ b' \circ b''$ were placed parallel to each other their ends would coincide. Likewise for any rational magnitudes of attributes, e.g. ‘ a is 1.5 times the length of b ’ means that b concatenated with half a copy of b will be as long as a . Things like halves (and indeed any fraction) of units can be defined using congruence: e.g. a unit x is half of y when if an y is divided into two y' and y'' , x is congruent to both y' and y'' . And so on for more complicated rational fractions of empirical magnitudes and units of them e.g. a mass is 1.567 kg if it has the same mass as $(1 \text{ kg}) \circ (0.5 \text{ kg}) \circ (0.06 \text{ kg}) \circ (0.007 \text{ kg})$. However irrational magnitudes cannot be so treated, since irrational numbers have infinite non-recurring decimal expansions. Kyburg points out that “[i]rrational magnitudes present a problem in general. They are clearly included among the values that are talked about in the equations of physics, yet no measurement can yield an irrational value” (Kyburg 1997, p.389). I acknowledge this problem here, but this thesis is not the place to discuss the issue, which as a general problem in the philosophy of science does not pose a particular problem for the mapping account of applicability.

As well as products, ratios are also used to represent empirical relationships, and are described as quotient relationships. The relation here is similar to product, but crucially different. Take for instance the derived attribute of electrical resistance of a circuit, defined in terms of a relation of the voltage across a circuit to the current flowing through that circuit. The empirical relationship is one, you might say, of 'empirical ratio' or 'empirical inverse proportion'. It is that for resistance to remain the same if the voltage or current doubles then the other magnitude must double as well, and if the voltage or current halves then the other magnitude must also halve. Again note that the ratio *represents* an empirical relationship, it is not an empirical relationship, although common usage might disguise this fact: we are used to saying that resistance is the ratio of voltage to current, where properly speaking we should say that the number representing resistance is the ratio of the number representing voltage to the number representing current, if numerical ratio is what is meant.

Thus we see (from 2.2.) how useful mathematics is in representing fundamental empirical attributes, assuming a robust unit of measurement is provided, and we also see from this subsection the importance of mathematics in representing derived empirical attributes, depending on relations between fundamental attributes. In neither case is there any problem for the mapping account. So long as there is a representation theorem showing that, subject to empirical structures satisfying certain empirical axioms there is a structure-preserving mapping from the ordered empirical domain plus some empirical relation to a domain of mathematical objects and an operation on them, such as addition, subtraction, multiplication or division and so on, it is clear that the role of mathematics here need only be representational and that the mapping account is sufficient to explain this. We will soon see that this sufficiency extends, fairly trivially, to laws as well.

2.3.2 Derived Attributes and Natural Laws.

This business about derived attributes is all very well, the reader may think, but what of scientific, or natural, laws, the real subject matter of science? It may seem initially surprising, but once we have an account of magnitudes of derived attributes, there is very little remaining to be done in the case of laws. For 'laws', as we shall see, are just how we refer to more complicated definitions of derived magnitudes. Once we have the concept of a derived attribute of some object as a functional relationship between other attributes of that object, we can generalise to a concept of a derived attribute of multiple objects as a functional relationship between the other attributes of multiple objects. This is not to deny that a sort of distinction is often made between derived attributes and laws, but it is to deny that such a distinction is well-grounded.

So what is the perceived, though actually non-obtaining, difference between calculating the magnitude of a derived attribute, and making a prediction using a law? Or, in other words, what is the difference between a definition of a derived attribute and a law, since both have the form $a = b R c$ (or some more complex form)? The answer is that definitions of derived attributes usually concern attributes of the same object (e.g. the force acting on an object a is a relation between the mass of a and the acceleration of a) but laws, by contrast, relate attributes of *different* objects. The law of universal gravitation for example relates two massive objects a and b . The attributes that are related are the mass of a , the mass of b , the length (the distance) between a and b , and G , the universal gravitational constant. The relation in question is 'gravitational attraction', which is a derived attribute defined as a relation between attributes of diverse objects, not just attributes of the same object.

So we see that the distinction between derived attribute and law is not really a difference in kind but is rather a purely psychological distinction and moreover one that may be fluid over time. Thus there is no fundamental difference between the application of a law to yield the prediction of a magnitude and the calculation of the magnitude of a derived attribute. Furthermore, if it is accepted that the primary purpose of laws is not to make predictions but to state how phenomena are related,

a philosopher may claim that the definitions of derived attributes, insofar as they state relationships between attributes of the same object are as much laws as those statements we consider to be laws which also state relations between attributes, albeit of multiple objects. This point must be granted, but since the use of the term 'law' for a certain class of functional relationships is widespread in the literature, I shall retain it in what follows.

One important role of mathematics in relation to science is that because mathematics enables us to state the magnitudes of empirical attributes accurately and concisely it gives a very clear indication of the magnitudes of the relata of empirical relationships generally and laws in particular, making it much easier for a scientist to perceive and test putative laws than would be the case without clear measurement of such magnitudes, a key step in the discovery of a law. In many cases, certainly those examples from before the development of modern mathematical physics, the basis, the impetus, of such laws is observation of some sort of regularity. In modern theoretical physics it may be that some sort of mathematical analogy as well as observation can motivate a suspicion of the existence of a law – I will say more on this, and its ramifications for the philosophy of applied mathematics, in chapter five. When scientists have reason to believe that laws exist they investigate, partly through making predictions (which usually involve some sort of experiment) to see whether the putative laws are consistent with the known and observed empirical data.²⁶ What is important is that whatever reason a scientist has for suspecting the existence of a law, his putative law decides

²⁶ The laws of nature themselves are not just vague regularities, but are in fact very precise, and it is part of the business of science to make statements of laws as precise as possible. Of course the scientist may well, and indeed should, be sceptical about his ability to state a law of nature in full, that is genuine, detail. But this will not discourage him however from attempting to get ever closer to the law through more detailed measurement, more fine-grained statements of relationships between the empirical attributes with which the law is concerned, and so on. It is just this detail that ensures that putative laws can be discriminated adequately and rejected if they fail ever more stringent tests. For instance, both General Relativity and Newtonian Mechanics hold that it is a law of nature is that gravitational attraction is proportional to mass. But they disagree over exactly what that proportionality consists in, and so although the rough informal statement may be a statement of the law on some level, it is not adequate as it is consistent with misstatements of the law and cannot deliver precise predictions.

an empirical relationship between empirical attributes. Thus the content of the law will be empirical content.

However our usual empirical vocabulary is too clumsy and impossible to use without getting bogged down in unnecessary details to describe relationships clearly and precisely. Instead some more precise language is required, such as the language of classical mathematics. The subject matter of this language does not have to be abstract objects in the usual platonistic sense, though that is the standard view of the semantics of mathematics. Some sort of nominalistic language will be perfectly acceptable, as will nominalistic values of measurement, so long as they are able to provide the precision that is required, to something like the level of the precision provided by classical mathematics. In chapter four I look at ways that references to abstract mathematical objects can be removed from science, but that is not important at present.

For now I want to avoid the issue of mathematical ontology, and will for convenience treat mathematics in its classical sense. So mathematics is useful partly because it is more precise than non-mathematical language. The mathematical form of a law relates various numbers or other mathematical objects, which represent the previously measured magnitudes of the attributes the law relates and the magnitudes that are to be compared with the empirical data, with reality, when the law is used to make a prediction. We have seen above, at the risk of being repetitive, how the mapping account (and measurement theory) shows that mathematics can represent these magnitudes, and that its precision here arises partly from the fact that it can represent any physical unit, no matter how small, even if physical objects get smaller and smaller all the way down, past quarks and so on, *ad infinitum*, because the real numbers, for instance, are dense.

The more pressing question is ‘why is mathematics so useful in stating the *relationships* of attributes, and how does it do so?’. The reason for this at least is straightforward. Mathematics is designed to focus exclusively on only certain features of objects or structures, to abstract from many of their specifics. For instance applied geometry concerns (partly) angles of empirical phenomena, but only what is relevant to the angles being the angles they are, the angle properties,

not any of the other properties of whatever it is that happens to be instantiating the angle. When counting how many objects there are in a discrete collection we are concerned with how many of them there are, not with what their colour, shape, etc happens to be. In order to be precise laws have to focus on some aspects and not others. Specifically they want to ignore all attributes of any objects or relations the law concerns that are not relevant to the phenomena being described. And mathematics, designed as it is to abstract from objects and structures, is perfect for this descriptive and representational task, as it is able to focus precisely on only certain, frequently functional, relations of phenomena. I shall now illustrate a particular example of this, Newton's law of universal gravitation. I shall sketch tentatively and speculatively how the knowledge of such a law came about, how mathematics was used in its description, why this was important, and why the role of this mathematics need only be viewed a representational one.

2.3.3 A Speculative Illustration of Mathematics Applied to a Law.

The example in question is Newton's discovery of the law of gravitation. In order to avoid being bogged down in extensive biographical and historical details I am going to sketch how this may have happened. This sketch is an illustration, to motivate this idea that there is no *a priori* reason to think that the mathematics involved in the discovery of laws plays anything more than a representational role. The illustration is therefore not exactly rigorous, but I do think it informative, once its limitations are borne in mind. Let us assume that through observation, either informal observation, or through a study of measured magnitudes, Newton noticed that 'attraction between two bodies seems stronger the more massive they are and weaker the further away they are'. His interest was piqued, and he believed that there may be a law at work here concerning this attractive force. It is clear for the moment what the objects interacting with the force seem to be, and what their relevant attributes are, namely mass and distance. Assuming that there is such a force Newton ponders how it propagates. Given that it gets weaker with increased

distance it must be the case that it is inversely proportional to the distance (this simply means its strength

. decreases with an increase in distance). So far so good, no mathematics has been required except to measure what is being observed, an application of mathematics which the mapping account has already accounted for. At the moment Newton thinks his law is that 'gravitational attraction increases with the mass of the objects and decreases with the increase in their distance', or put somewhat more precisely 'gravitational attraction is proportional to the mass of the objects and inversely proportional to their distance'. Newton believes that this has a good claim to be a law, and although it may be that a significant amount of general observation establishes the plausible lawfulness of this statement, or at least makes it a serious contender for being a law, in order to confirm it to within standards of scientific rigour Newton needs to test it by making predictions. However in order to make a clear and unambiguous prediction Newton has first to get it into a form that can handle the precise measurements he has made of the initial conditions. That is, he needs to get the law into a mathematical form.

How does he go about this? Well, obviously he needs symbols for the masses of the two objects and their distance apart. Given that the masses are not being in any way concatenated, Newton cannot represent their relation additively, and he does want to convey the fact that strength of gravitational attraction is directly proportional to the masses. This empirical relationship of the two masses can be represented mathematically using multiplication, $m_1 \times m_2$. To express the direct proportionality therefore Newton writes $F \propto m_1 \times m_2$. But this cannot be the whole story for the distance between the masses has not yet been taken into account, and must be so if an accurate prediction is to be made. Now, Newton has already observed that the effect of increasing the distance between two masses reduces the gravitational attraction between them. This is easily represented by dividing the product by the distance, as the denominator gets larger the result of the division gets smaller.

Once he has brought distance in, the only other variable that seems to affect gravitational force, Newton thinks he has the entire story about what is going on, that by allowing distance to alter the proportion of gravitational force to mass he can now account exactly for the relationship between gravitational force and mass. In other words, he believes that empirical relationship between gravitational attraction, masses m_1 and m_2 , and distance r can be represented mathematically by the following equation:

$$F = \frac{m_1 \times m_2}{r}$$

Now, suppose Newton uses this lawlike statement to make some predictions. The observed and measured empirical data do not agree with his predictions, and he realises that there must be something his law has not accounted for, something else affecting the proportionality of gravitational force to mass.

Reflecting on what this could be, perhaps with or without further empirical observation, it occurs to him that maybe the force does not connect the two masses like a line, but rather that force spreads out from each mass, each affecting the other. That is to say, as the force spreads out over a greater distance from its source, it affects a larger and larger area more and more weakly. Thus it is not just the distance of the force that affects its strength, but the area it covers, which is itself dependent on that distance. Drawing upon his knowledge of geometry as the science of space, Newton recalls that any mathematical expression of the area that a plane covers, in terms of dimensions given in some unit, involves the square of those dimensions in terms of that unit.²⁷ If his law is to account for the weakening of force over an area the mathematical representation of this must involve the square of some dimension. The question is, what is this dimension? Well, the force spreads

²⁷ Here I am treating geometry as a physical science of physical space, albeit a heavily formalised one, that originally had a purely empirical treatment, before the development of formal (analytic) methods. To a degree this is true, and was the accepted view in Newton's day. Nowadays of course geometry is a branch of mathematics not a physical science, it is at best applied mathematics, but this does not affect the mapping account as the relation of geometrical objects to physical objects is easily explicable in terms of instantiation.

out uniformly as the distance increases, so to some extent the area affected will depend on the distance travelled. If the distance travelled is one unit of length, the area affected will be (partly) accounted for in terms of that length. Thus we say the force decreases not just with the length of the distance, but with its square, that is r^2 . This image conveys that perhaps more clearly than my statement:

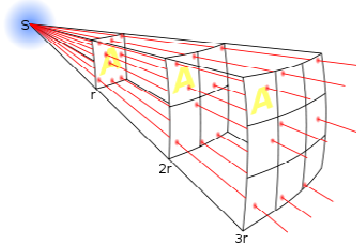


image source: en.wikipedia.org

The mathematical form of his law is now:

$$F = \frac{m_1 \times m_2}{r^2}$$

This means that say e.g. the two masses are 4 and 5 kg respectively, and the distance is 3 metres, then the gravitational force will be 2.2 something. Naming the derived unit of gravitational force the Newton, Newton can say the gravitational force is 2.2 Newtons. By his former statement of the law it would have been calculated as 6.6 N. In other words by his new law, gravitational attraction is much weaker than his old law had suggested. Newton again makes his predictions and discovers that the new predictions agree much more closely with the data. Although the agreement is not exact the difference is extremely small. This suggests that the law is nearly correct, but since it is not completely confirmed something must be missing. Newton tries to consider what but is at a loss – he seems to have accounted for all the variables. However he notices additionally that all his predictions are off by the *same* proportion, suggesting that there is one *constant* attribute affecting the proportionality of gravitational force to mass which he has not taken into account.

Calculating its value in relation to the observed data, Newton introduces it as a constant of proportionality to correct his predictions, arriving at a refined version of his law:

$$F = G \frac{m_1 \times m_2}{r^2}$$

At this stage this is nothing more than an instrumental way of getting his laws to deliver correct predictions, but it is itself a promissory note – it says that something needs to be explained, something we know the *magnitude* of, but not *what* it is. This would not have concerned Newton who took himself to be describing rather than explaining phenomena, and so as long a law yielded correct predictions that would have been enough for him. What is important to note is that the universal gravitational constant is not an abstract mathematical object, but a physical phenomenon. It is not that an abstract mathematical object is interacting with the physical world to affect gravitational force, but that we can measure the magnitude of the attribute without being able to point to its source.

We see therefore that the role of mathematics in the discovery and statement of laws in this sketch did not go beyond anything merely representational – at no point had mathematics done anything more than represent the magnitudes of empirical attributes, and represent the relations of those attributes' magnitudes. Insofar as the mapping account says that the applicability of mathematics consists in its ability to represent empirical attributes and their relations (that is, functional empirical relationships) and that this representation obtains because of various mappings between empirical objects (or collections of them) and mathematical objects, it is clear that the mapping account can explain the role of mathematics in the discovery and description of laws, if those laws are at all as I have described. In chapter five I confront an attempt to show that this representational role of mathematics does not extend to all laws, but I hope I have done enough to benefit from the reader's tentative agreement that this is not the case. With this settled, I shall now look at an account that has been proposed as an advance on the mapping

account as conceived in this thesis, the inferential conception of the applicability of mathematics.

2.4 The Inferential Account

2.4.1. What is the Inferential Account?

I explained at the beginning of this chapter, and indeed earlier in the thesis, that until very recently no fully worked out mapping account of the applicability of mathematics has appeared in the literature, rather there have been a flurry of papers containing a brief account of the applicability of mathematics in terms of mappings and involving one or two simple examples generally drawn from measurement theory, thereby leaving a fully worked out mapping account as a task to be undertaken in the future. It is certainly the case that a full length treatment of the account is required, and into this vacuum, with their recent paper ‘An Inferential Conception of the Application of Mathematics’ (2011), have stepped Otavio Bueno and Mark Colyvan, who have argued that the mapping account does not go far enough to account for the diverse applications of mathematics.

Insofar as the account has not really been hitherto adequately developed this is difficult to argue with, and so the point would have to be granted, though it would be somewhat vacuous. However, since a key part of my thesis is just such a development of the mapping account, as given in this chapter, and in chapters five (examining the role of mathematics in the making of novel empirical discoveries) and six (looking at, among other things, the role of idealisations), I shall interpret Bueno’s and Colyvan’s remarks *in the context of this thesis*. This may seem unfair – after all their paper was published before my thesis was even finished – but it is important to see if the mapping account can stand up to the criticisms that Bueno and Colyvan make of it, in order to see whether those criticisms apply to the mapping account only in its nascent state, if at all. If they do only apply to the

mapping account in that state, which is what I shall argue, then it is clear that the criticisms are of little import. So what is the inferential conception of the applicability of mathematics?

The primary claim of the inferential account is that the fundamental role of mathematics is inferential. But what does this mean? Bueno and Colyvan suggest the following: “by embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extremely hard...to obtain” (Bueno and Colyvan 2011, p.352). Thus applicability is to be explained in terms of inferential roles between mathematical and empirical structures. Even though there are diverse roles of applied mathematics – the unification of scientific theories, the making of empirical predictions, etc, Bueno and Colyvan maintain that “all of these roles...are ultimately tied to the ability to establish *inferential relations* between empirical phenomena and mathematical structures, or among mathematical structures themselves” (Ibid.) The inferential account is described as consisting of three stages:

- i) *Immersion*. Establish a mapping from the empirical to the mathematical. Several mappings may do the job so we choose the best one contextually.
- ii) *Derivation*. Draw consequences from the mathematical formalism and the structure into which the empirical phenomena have been mapped.
- iii) *Interpretation*. Interpret the mathematical consequences in terms of the initial empirical set up. This requires a mapping from the mathematical to the empirical which likely will not be (unless it is an isomorphism) the inverse of the mapping used in the immersion stage.

2.4.2. Putative Advantages of the Inferential Account over the Mapping Account.

Bueno and Colyvan are emphatic that the inferential account offers many advantages over the standard mapping account, and in both section three of their paper, and *passim*, they list various such advantages. I have so far said nothing to suggest that the inferential conception may not be to some degree an important account of applicability in its own right, as a rival to the mapping account. I do not think that can be the case. For the inferential account is not, and nor is it intended to be, an *entirely* new account of the applicability of mathematics - rather Bueno and Colyvan wish to significantly extend or modify the mapping account of the applicability of mathematics.

Straightaway this raises some issues, for if Bueno and Colyvan are extending the mapping account they must believe it stands in need of development, that is, is underdeveloped. Thus they would be wrong to present their ‘inferential conception’ as rival to the mapping account, and would be better to present it as one way of developing it, for no philosopher remotely familiar with the account would hold that the mapping account has received an adequate treatment in the literature thus far. As I said briefly above, if the ‘standard’ mapping account is the account that has been presented somewhat scantily in the literature then some of Bueno and Colyvan’s criticisms of it, or claims to have advantages over it, are uncontroversial. However if the mapping account developed in this thesis is a more refined version of the standard account, then (I argue) Bueno and Colyvan’s criticisms fall short of their target, and that the advantages they claim for the inferential account are by no means unique to it. I shall consider these criticisms and putative advantages in turn.

Claim 1: The mapping account says very little. “According to the mapping account of mathematical application...the idea is that there is some sort of structure-preserving mapping between the world and the mathematical structure...and that is pretty much the end of it” (Bueno and Colyvan 2011, p.347) and “until the central

notion of mapping is clarified, the account is little more than a gesture..." (Bueno and Colyvan 2011, p.348).

I have argued above that the best way to view the mapping is as an isomorphism (partial or otherwise) between equivalence classes of empirical objects and the real numbers, for example. Moreover, insofar as measurement theory can be taken to be an important part of the mapping account, Bueno and Colyvan's claim can be seen to be false, as measurement theory pays great attention to what sort of mapping, what sort of homomorphism, is required between an empirical relational structure and a mathematical structure.

Claim 2: The mapping account is incomplete. "...crucial information required to solve [a] physical problem is not part of the mapping between a mathematical structure and a physical structure. In short, the mapping account...is incomplete". (Bueno and Colyvan 2011, p.349).

Bueno and Colyvan explain that one important example of what they mean by this alleged incompleteness is that the mapping account does not tell us which solutions of an equation will be physically real. The only case that Bueno and Colyvan give in any detail to substantiate this claim is the using of a quadratic equation to calculate (that is, predict) the displacement of a projectile.²⁸ The quadratic has two solutions, but only one is physically real. Applied mathematicians and military engineers use such an equation to calculate e.g. a shell's position, but they use background information *not* contained in the equation to discriminate between the true and the false prediction, the physically real and the non-physically real solution. However that is no reason to expect philosophers to be able to say *a priori* what solutions will and will not be physically real, especially as what is and is not viewed as physically real in physics changes as science progresses. Rather it is trivial that not all of the information required to solve a physical problem is part of the mapping account,

²⁸ *This* does not undermine their argument, as there are clearly a great many examples where it is not clear which solutions are physically real. As we will see, this is not a fact I am taking issue with, but rather the relevance of this to the mapping account.

and indeed we would not expect that account to contain the information required to solve all the physical problems insofar as there are a great many different applications of mathematics to physical science, each individual application depending on the circumstances in which it is being made. One would not expect of the mapping account that it should be able to specify each specific case: it says that once the relevant empirical attributes have been identified, it is possible to map these into a mathematical structure, plug these values into the mathematical form of a law, and obtain a mathematical result with a clear interpretation in terms of the attributes the law relates.

For the mapping account and measurement theory tell us in general what sort of axioms an empirical structure has to satisfy in order for the mathematics to be applied, and the answer obtained. But questions about what attributes are to be identified or what sort of additional restrictions (whether formulated as axioms or not) are to be imposed on the empirical structures is a matter left to scientists, not philosophers of mathematics. What is important is that there *are* empirical attributes, that these *do* have a structure, and that this *can* be mapped into a mathematical structure if certain axioms are satisfied, and that we can classify these structures, mappings and axioms and discuss their philosophical implications. It would be unreasonable to expect a philosophical account to tell us how to solve specific physical problems, and any account that could do so would be extremely large and unwieldy, not because it contained a great deal of philosophy, but because it would have to contain a great quantity of empirical data and material not properly belonging to philosophy. As such the mapping account cannot be considered incomplete for the reason given above.

Claim 3: The mapping account cannot deal with novel predictions or discoveries. “The unreasonable effectiveness of mathematics...the mapping account of applied mathematics provides no solution [to this question...but the inferential account does]” (Bueno and Colyvan 2011, p.350).

I do not address this ‘unreasonable effectiveness’ until chapter five so I must ask the reader’s indulgence until then, though the reader may turn to chapter five now if she wishes, as it stands more or less alone. However I will give a brief summary of my position in that chapter, where I classify three categories of (descriptive) problems of such ‘unreasonable’ effectiveness, and argue that each of them can be treated by the mapping account. (Please note that the majority of what is discussed here is merely a skeletal framework into which material is inserted in chapter five). A descriptive problem of the first category is generated when we take an equation describing one phenomenon, manipulate it mathematically, and obtain another equation describing another phenomenon, which makes it look like mathematics has some power to give us knowledge of the world by non-trivially transforming our existing descriptions of the world into other descriptions of different parts of the world. Descriptive problems of the second category are generated when a scientist finds a solution to his equation that normal science rules is unlikely. The descriptive problems of the third category are generated by the pre-existence of mathematical theories which are vital to the statement of a given theory, or to the prediction of a given phenomenon, but which are taken ‘off the shelf’ as it were, in that they were not created by the scientist for the purposes of science, but rather by the pure mathematician for the purposes of mathematics.

My response in brief to these categories is that first category problems occur when describing similar phenomena, and we ought not be surprised if an equation (that is, formalised law) describing one phenomenon were similar to another equation (formalised law) describing a similar phenomenon. There is nothing here that suggests the role of mathematics is anything more than representational, and I challenge the advocates of the inferential account to say otherwise. As far as second category problems are concerned, this is explicable in terms of the physical intuition of scientists, honed by years of training. Finally, third category problems can also be easily met: since both mathematicians and physicists spend all their time looking at very abstract structures, and mathematicians prove thousands and thousands of theorems about mathematical structures every year, it would be more surprising if the physicist did not find on occasion that the mathematician ‘had been there before

him'. It seems then that in the case of second and third category descriptive problems there is no case for the mapping account, or indeed any other account of applicability, to answer.

To avoid the charge of being overly dismissive however I do want to look at the one example that Bueno and Colyvan actually give – time dilation. They say

Consider, for example, time dilation in Lorentz's (1904) pre-special relativity theory of length contraction. Lorentz seemed to take the time-dilation effect as a mere artefact of the mathematics—that is, not physically significant—presumably because of rather natural (pre-relativistic) intuitions about the nature of time. As we now know, Lorentz was wrong about this feature of the mathematics being an artefact. (Bueno and Colyvan, p.350)

Lorentz wanted to explain the failure of the Michelson-Morley experiment, and appealed to length contraction to do so. He hypothesised in 1895 that when an object is moving its length appears to contract in the direction of motion relative to the observer in an inertial frame, specifically that contraction will be inversely proportional to the Lorentz factor γ , thus $(L = \frac{L_0}{\gamma})$ where L_0 is length of the object measured in the moving frame. The Lorentz factor is

$$\frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}$$

This factor is also vital in the description of time dilation, which occurs when time of by a moving object 'slows down' relative to the observer. Time dilation was described as $(T = T_0\gamma)$, and has been experimentally verified in the motion of muons. However what Bueno and Colyvan are taking issue with is not that the Lorentz factor appears in both equations, indeed given that both time dilation and length contraction are relativistic phenomena involving a change to an object relative to an inertial frame at high velocity, we would expect them to be described similarly. Rather Bueno and Colyvan are expressing surprise at the fact that the time-dilation that is a consequence of Lorentz's theory, because his inclusion of the so-called 'local time' $t' = t - x \frac{v}{c^2}$ to make it easier to calculate processes for moving frames turned out to be real despite that fact that the equation leading to it was included only for

simplicity and the fact that at the time Lorentz did not believe in time dilation. Lorentz wanted to ensure the speed of light would be invariant across different inertial frames. This is why time dilation appears to be a mere artefact. Of course post-Einstein we know that time dilation is not just an artefact, but formulating his transformations that would enable the speed of light to be viewed as invariant even as late as 1904, Lorentz could not have known this. So how do we explain that this happened?

In the classification above, this is characterised as a ‘second category problem’ – something a theory suggested existed but which we had reason to disbelieve in turned out to exist. I have no wish to explain chapter five in too much detail here, to avoid repetition, but I do think that this is explicable in terms of the fact that the reason the addition of the local time in this instance made the calculations simpler is because the local time, and the time-dilation that fell out of it, was, unbeknownst to Lorentz, a physically real phenomenon. Lorentz had to involve something he disbelieved in in his calculations to make them work, but the fact that he had to describe such a thing to make the rest of the theory work suggested that thing actually existed. There is nothing irredeemably non-representational going on here, since the equations in question directly describe empirical phenomena. The local time may have appeared to be an artefact, but insofar as it talked about time, velocities and the speed of light it was an empirical artefact, a ‘heuristic working hypothesis’²⁹ and one that Lorentz would have hoped to at some point remove. Bueno and Colyvan do not go into much more detail about the ‘unreasonable effectiveness’, so there is little more to say here.

Claim 4: The mapping account cannot handle structural mismatches. “Another related problem with the mapping account occurs in cases when there is known mismatch between the empirical structure and the mathematical structure” (Bueno and Colyvan 2011, p.351).

²⁹ http://en.wikipedia.org/wiki/History_of_Lorentz_transformations

This is the problem that idealisations pose for the mapping account, or indeed any account of applicability. Bueno and Colyvan suggest the inferential account can deal with this the following way, by invoking ‘partial mappings’, between the empirical and mathematical – these partial mappings are the idealisations, “in contexts where idealisations are employed, the existence of a partial mapping...explains in which respects the idealisations work. The latter capture certain elements of the actual world, but not all of them”. (Bueno and Colyvan 2011, p.358). If idealisations do pose a problem for the mapping account then this is serious. I address this issue in chapter six, section 6.3. As with chapter five, this section stands largely alone, so the curious reader can turn if they wish to that section now to glance at it briefly. But to forestall the need for this I shall again provide a brief summary of my position. In chapter six I consider two putative sorts of idealisation, mathematical and physical. I argue that although if mathematical idealisations existed they would be damaging to the mapping account, there is in fact no evidence of such idealisations, and argue against Batterman’s (2010) paper promoting belief in such idealisations. Regarding physical idealisations, I claim that physical idealisations are simplifying assumptions that describe an empirical structure that strictly speaking does not exist, but about which it is easier to perform calculations whilst delivering a result sufficiently close to the observed magnitude to be useful. The mapping account has only to say how the ideal structure is mapped into the real numbers for example, and that is the same as any other empirical structure that is so mapped, in the manner described above in section 2.2. The advocate of the mapping account does not need to say how idealisations are possible, as this is an enquiry belonging to the philosophy of science. This is not to say that a treatment of idealisations in terms of partial mappings could not be an interesting or even potentially useful exercise, but it is to say that it is not an essential one for the advocate of the mapping account.

Claim 5: The mapping account cannot handle genuine mathematical explanations. “If mathematics is genuinely explanatory...this will present a problem for the mapping account”. (Bueno and Colyvan 2011, p.351).

I assume by this Bueno and Colyvan mean ‘genuine mathematical explanations of empirical phenomena’ in the sense of ‘genuine platonistic explanations of empirical phenomena’ and in the next chapter (three) I will argue against the existence of such explanations.³⁰ Given this, I would argue that the inability of the mapping account to account for genuine platonistic explanations of physical phenomena is a bonus as it means that there is no way to support this false supposition from the mapping account. If instead Bueno and Colyvan refer to *mathematical* explanations of *mathematical* facts, then this issue passes the mapping account, and any other theory of the applicability of mathematics to *empirical* phenomena, by.

Claim 6: The mapping account lacks an interpretation step. “There is one major and obvious difference [between the mapping and inferential accounts], and that is step three. There is nothing that resembles this step in the mapping account”. (Bueno and Colyvan 2011, p.354).

This is simply untrue: if the mapping account did not have some sort of an interpretation step it would be useless as an account of applicability, since it would not have a step for moving from the mathematical structure back to the empirical structure, with meaningful empirical results. However the mapping account’s interpretation step does not need as much freedom as that of the inferential conception since for the standard mapping account the use of equivalence classes means the mapping is an isomorphism (partial or full), so there is no need to decide at the interpretation stage which ‘downward’ mapping to use, we simply use the inverse of the isomorphism that took the empirical structure to the mathematical structure in the first place, as shown in subsection 2.2.5 above.

³⁰ I shall therefore not provide a summary here as the discussion will be introduced in several pages time.

2.4.3. Remarks Concerning the Inferential Account.

I have shown how the mapping account may resist the criticisms levelled at it by Bueno and Colyvan. Moreover in many crucial areas there are in fact great similarities between the two accounts, for instance the three stages of the inferential account do not differ significantly from anything in the mapping account, which also involves the possibility of making some choices at the ‘immersion’ stage, concerning e.g. what mathematical structure to map into. Likewise the mapping account and inferential account both involve a derivation stage where mathematical consequences are derived, and a stage where these consequences are interpreted empirically using a mapping (in this case the inverse of an isomorphism). However I have argued that suitably developed the mapping account has several advantages that the inferential account lacks. These are that unlike the inferential account, the mapping account is not burdened by the need to provide complete information to solve every physical problem; it is very clear about what mapping is required from the mathematical back to the empirical; it avoids any commitment to dubious ‘genuine’ platonistic explanations of empirical phenomena; and it does not provide an unnecessary account of idealisation. I submit that the version of the mapping account as developed in this thesis is an advance over Bueno and Colyvan’s inferential account. I hope that the treatment that the mapping account has received in this chapter has been sufficient to make the nature of that account, and its potential, clear. In the following chapter we turn to a discussion of putative genuine platonistic explanations of physical phenomena.

Chapter 3

...even on the platonistic assumption there are numbers, no one thinks those numbers are causally relevant to the physical phenomena.

– Hartry Field (1980)

The Dispensability of Platonistic Explanations of Empirical Phenomena.

In this chapter I am concerned with arguing against genuine platonistic explanations of physical, that is empirical, phenomena. This is essential to the mapping account as if such explanations were possible an account of applicability would need to explain how this could be the case, which the mapping account does not do, implying the mapping account would be either straightforwardly false or radically incomplete. Section 3.1. is the chapter introduction. Section 3.2 discusses the nature of explanation in general and causal explanation in particular. 3.3 and 3.4. are concerned with describing Baker's cicada example of a genuine platonistic explanation of a physical phenomenon and examining arguments against it, formulating a nominalistic version of the explanation of the cicada example and showing therefore that platonistic mathematics is dispensable to Baker's example. I conclude that Baker's failure does not bode well for genuine platonistic explanations of physical phenomena, and that if there is no need for such explanations, the mapping account has no reason to accommodate them.

3.1. Chapter Introduction

Why does a thesis defending a mapping account of the applicability of mathematics to empirical phenomena require a digression into genuine platonistic explanations of such phenomena? The answer is simple: if there are genuine platonistic explanations of physical phenomena, the mapping account's failure to address this, *qua* theory of the applicability of mathematics, would render it unacceptably incomplete or inadequate. It is not so much that the account would lack a detailed explanation of how the platonistic could explain the empirical, but rather that it would not address this issue at all. If there is no need for such explanations then this requirement is obviated, and an explanation of applicability can focus on the representational abilities of mathematics, as was undertaken in the previous chapter. Thus the aim of this chapter is to indicate what form a theory of applicability should, or need, *not* take, and to justify the conception of applicability as purely a matter of the *representation* of empirical phenomena.

Aside from its implications for any theory of applicability, the discovery of a genuine platonistic explanation of a physical phenomenon would also be a holy grail for a platonistically inclined philosopher whose justifications for mathematical platonism centred around the indispensability argument (see chapter six). This discovery would be so important because it would imply that platonistic mathematics is not just psychologically indispensable, or practically indispensable, to explanations of physical phenomena, but rather that it is genuinely indispensable, and would therefore provide complete support to the second premise of the indispensability argument. The purpose of the discussion of platonistic explanation in this chapter is to assist in arguing against such genuine platonistic explanations of physical phenomena. Alan Baker has provided the most widely cited example of a

such an explanation in the literature, which concerns the prime lifecycles of periodical cicadas.³¹

My response to Baker takes the form of several arguments, some appropriated from the literature, and one of my own. If these arguments are successful then neither platonistic objects nor platonistic concepts are part of the genuine explanation of why cicadas have prime lifecycles, and thus such concepts and objects *must* be dispensable to any genuine explanation of this phenomenon. As Baker's is perhaps the best extant example of a putative genuine platonistic explanation, I conclude that the success of my argument against Baker shows that the burden of proof is on the proponent of genuine platonistic explanations to find an example of a platonistic explanation of a physical phenomenon to which the criticisms levelled at Baker do not apply. I suggest that this burden is too heavy for such a proponent to carry. Of course there are many good explanations of physical phenomena which do contain platonistic mathematics, indeed this is likely true of the majority of such explanations, and it may even be the case that human cognition is such that without platonistic mathematics we cannot even practice natural or social science, that such mathematics is indispensable to human scientists' explanations even if it is not indispensable to all possible good explanations of the phenomenon in question. This however does not mean that platonistic mathematics should be considered part of the *genuine* explanation of a physical phenomenon, at least not if it can be shown that this mathematics is dispensable to a given explanation.

3.2 Explanation and Genuine Platonistic Explanation

An explanation E of a fact f_0 is a statement describing a relation R of further facts $f_1...f_n$ such that f_0 *obtains* because $Rf_1...f_n$ obtains. For example, suppose the fact we

³¹ Several examples have been given, but Baker's focuses purely on number-theoretic rather than geometrical examples, and thus bypasses the accusation that geometry is an empirical science of space rather than a platonistic theory, and that therefore genuine explanations involving geometry are in fact empirical explanations of empirical phenomena.

want to explain, f_0 , is 'precipitation takes place at a particular location x '. The explanation of this phenomenon appeals to a relation R between chemical facts (e.g. the condensation point of water), geological facts (e.g. the proximity of the location to mountains), and environmental facts (e.g. the temperature and humidity, and the presence of nearby water sources). Explanations are vital because without them we would not know, or at least believe we know, why or how things occur, just *that* they do. Explanation has not always been so highly desired in science – consider Newton's injunction to 'feign no hypotheses' and merely describe phenomena. This was due to his belief that this method was more reliable for making accurate predictions than the messy, speculative, physics that had dominated natural philosophy almost exclusively prior to the era of Copernicus and Galileo.

The subsequent development of robust scientific methods has ensured that explanation can be sought through the discovery of laws, or putative laws, that can be verified experimentally. It is possible to provide an explanation that *seems* to account for why f_0 obtains but which does not do so in reality, in the sense that the putative facts $f_1...f_n$ appealed to do not really bear a relation R to each other that actually accounts for f_0 . That is, the situation described by the explanation E does not comprise the actual relation of the 'real' facts of the situation, a failure which can be explained by the non-obtaining of (some of) the facts appealed to, or an inappropriate relation between the explanandum and the facts that are components of the explanans, or some combination of these. For instance, to return to the above example, I may try to explain precipitation through appeal to 'facts' about supernatural forces, but science tells us that this is not the genuine explanation of this phenomenon, not least because if we are disposed to believe in science we are likely to also be indisposed to accept the reality of empirically relevant supernatural facts. But the authority of science in such matters does not imply that scientific explanations are indubitable. Insofar as the majority of practising scientists are likely to be scientific realists they will be inclined to accept that any scientific explanation is in principle falsifiable and that one seemingly genuine explanation can be superseded by a superior explanation with a stronger claim to being genuine. In such an instance explanations may become increasingly refined and ever closer to

being genuine (we might even say they become ever more genuine, if that is not misleading) whilst remaining asymptotic to 'full genuineness'.

3.2.1. Causal Explanations

The main category of explanations of empirical phenomena that I accept are causal explanations, though we will see below that my conception of causal explanation is a fairly broad one, and does not just include the micro-causal explanations that are the business of physics. We have to be careful when discussing causal explanation precisely because there are a variety of sorts of causal explanation and if we ignore this and treat only those causal explanations involving just laws of physics (construed broadly enough to include chemical laws) our conception of causal explanation will be extremely impoverished. For instance, take an example of a fact that stands in need of explanation: birds fly. What is the explanation of this fact? One explanation will involve all sorts of facts about aerodynamics, gravity, bone density etc. This explanation of the fact that birds fly is an explanation of *how* any particular bird can fly, and will come out as a causal explanation on even a narrow conception of causal. But this is not the only explanation of the fact that birds fly. There will also be at least one other relevant sort of explanation, namely the explanation of *why* they can fly. Not why they *can* fly (that is just the 'how' question just considered above) but rather why it is they *do* fly rather than say, crawl.³² This too is an empirical question that stands in need of explanation, though clearly any explanation of this in terms of purely low-level physico-chemical facts is going to miss the point. Rather an *evolutionary* explanation must be provided.

An evolutionary explanation of why birds do fly is (I shall assume – see footnote 33) no less a causal explanation than the lower-level explanation concerning a particular bird and the story of how and why it can fly. It is admittedly

³² A similar sentiment is expressed in Colyvan (2010) p.302, concerning Kirkwood gaps in the asteroid belt. We can give a causal explanation in terms of forces, particles and the history of any given asteroid that explains why it does not happen to occupy a Kirkwood gap, but we also want to say why *no* asteroid will occupy such a gap. I agree, but disagree with Colyvan that we have to use a mathematical explanation to achieve this.

not a *microcausal* explanation, as it rather involves such things as selection pressures, extended time periods and fitness, but this does not mean it is not causal. The evolutionary explanation above concerning why birds fly, that they evolved the capacity for flight since a rudimentary ability to take-off gave a survival advantage, which has been reinforced and developed through hundreds of thousands of generations of birds (where this rudimentary ability can itself be explained in terms of further evolutionary facts which it is not necessary to go into here ³³) has, at the risk of hammering the point home too much, just as much claim to be an causal explanation of an empirical fact as the explanation in terms of bone-density etc, and will be correct if evolutionary theory is true. That is to say, it may have such a claim if evolutionary explanations do not involve essential reference to platonistic objects or concepts, *pace* Baker below who argues at least one evolutionary explanation does, in the periodical cicada case. Even if we are persuaded by Baker it does not follow that there are not any evolutionary explanations that are purely causal. *All I wanted to do was to motivate the idea that the category of causal explanations is not exhausted by the category of microcausal explanations, and that evolutionary explanations are a species of causal explanation.* This motivation was necessary to show that I am not simply objecting to Baker for the trivial reason that his proffered explanation is not a microcausal explanation, I am not just begging the question against him.

So what is a genuine explanation? A 'genuine' explanation describes how or why things actually happen in reality. As the remarks in the previous paragraph have indicated we cannot *know* that an explanation is genuine, since it is always possible that we are wrong. Indeed there may be a variety of competing explanations which can *appear* to explain x. If the explanations are of different types

³³ But in case the reader is curious, these are the options given in a Biology article on the topic: "Two fundamentally different theories have been put forward to account for the origin of flight in birds. One theory postulates an early adaptation to arboreal habits followed by successive adaptations that increased leaping ability and range, and facilitated safe, controlled descent from elevated positions...The alternative theory visualizes a primitive, cursorial, bipedal ancestral stock followed by succeeding stages that incorporated adaptations that augmented bipedal running and leaping up from the ground". (Ostrom, 1974, p.27). Nothing here suggests anything non-causal at work.

they may not be in competition, i.e. the low-level explanation of how birds fly and the evolutionary explanation of why they fly do not compete. Rather competition often occurs between theories of a particular type, i.e. two low-level explanations of a particular phenomenon and two evolutionary explanations. The issue is compounded by the existence of explanations with different levels of detail. An example of this is that gravity can be explained as ‘the force which attracts objects to each other in virtue of mass’ and also by General Relativity. Although we might like to say that both of these explanations are genuine, it is clear that the first explanation is also compatible with Newton’s law of universal gravitation, which is not compatible (except as a limit case in a particular form) with General Relativity. So the vaguer explanation can have both genuine and non-genuine instances, a situation which can only be avoided by an explanation being fully specific, though in practice this is unlikely to be possible, and we have to make do with the most genuine explanation of a phenomenon that we can obtain. The question for this chapter is, does this explanation ever have to be a platonistic one? I argue that it does not.

3.2.2. Platonistic Explanations of Platonistic Facts

I have included this section solely for completeness. Whatever view one holds of platonistic explanations of platonistic facts need not bear on the view of platonistic explanations of non-platonistic facts. The reader who wants a more thorough discussion would be advised to engage with some of the emerging literature on the topic. For instance, Marc Lange (2010) ‘What Are Mathematical Coincidences and Why Does it Matter?’, *Mind*, vol. 119, pp. 307-340, and Paolo Mancosu (2008) *The Philosophy of Mathematical Practice*, Oxford: OUP. In a platonistic explanation of a platonistic fact both explanandum and explanans would seem to have to be platonistic.³⁴ Such explanations abound. One view is that a proof of a platonistic-

³⁴ Such a fact may be explained *through* a physical example, but it is clear that with such an example, to quote Aristotle, we are treating physical magnitudes but not *as* physical. Diagrams for instance


mathematical theorem is an explanation of a platonistic-mathematical fact. For instance the explanation of there being no greatest prime number is may be thought to be explained by natural numbers being either prime or composite, together with there being no greatest natural number, and that the product of any collection of prime numbers plus unity is either a prime number or divisible by a prime number greater than any prime number which is a factor of the product. If these claims are correct then it follows there is no greatest prime number. If the reader is uncomfortable with the idea of a formal algebraic proof as an explanation there are more visual examples – for example, the triangular numbers. A triangular number is “the number of dots in an equilateral triangle evenly filled with dots...three dots can be arranged in a triangle; thus three is a triangular number. The n th triangular number is the number of dots in a triangle with n dots on a side”.³⁵ How do we explain the mathematical fact that ‘ $\frac{n^2 + n}{2}$ ’ is the number of dots that can be fitted into an equilateral triangle of dot-length n ’?

Well firstly, we bear in mind the expression for the area of a triangle, namely $\text{area} = \frac{1}{2} \times \text{base} \times \text{height}$. That is, it is the area of a rectangle with the base and height of the triangle, cut in half. Every rectangle can of course be cut in half to make two triangles, but only certain rectangles will yield *equilateral* triangles of dots if cut in half, that is, if the area is divided by two. These are just those rectangles one of whose sides is one unit longer than the other side, that is, those whose area (in terms of dots) is given by $(n \times n) + n$:



may be essential in grasping some mathematical fact, but no particular diagram is essential, and the diagram itself is irrelevant to the genuine explanation of the fact's obtaining. The mathematical fact would obtain without the diagram.

³⁵ http://en.wikipedia.org/wiki/Triangular_number.

The area of this rectangle, divided by two, that is, cut in half, will yield two identical triangles of $\frac{n^2+n}{2}$ dots, which is the simplified expression of $\frac{n(n+1)}{2}$, the number of dots in a rectangle of height n dots and length $n+1$ dots, cut in half. The triangular numbers are exhibited in the following dot pattern of triangles:  ... Thus the fact that the expression above yields the triangular numbers is explained in terms of further facts about areas, triangles, rectangles, and equivalent formulations of expressions.

Far more controversial is whether or not we can have genuine platonistic explanations, or partially platonistic explanations, of *physical*, that is, *empirical*, facts. *Prima facie*, it would seem we cannot. After all, as I said at the beginning of this chapter, if platonistic objects are causally inert, or do not exist, then how could they play a role in the genuine explanation of any physical phenomenon? For example, Hartry Field, the famous nominalist, says “...even on the platonistic assumption that there are numbers, no one thinks that those numbers are causally relevant to physical phenomena” (Field 1980 p.43). Even Baker, the philosopher advocating platonistic explanations of physical phenomena, does not think this: “[c]learly this [causal] account is incompatible with the existence of any *genuinely* mathematical explanations [of empirical phenomena], since mathematical objects if they exist are acausal” (Baker, 2005 p.234). However the force of this objection depends on the claim that the platonistic explanation involved merely concerns platonistic objects. Baker and Colyvan are making a stronger and more insidious claim however, that platonistic *concepts* are doing the explaining. Presumably platonistic concepts also do not feature in causal explanations, and as such Baker and Colyvan allow that there can be genuine explanations of empirical phenomena that are not causal explanations.³⁶ Below we shall see Baker try to show that there is an evolutionary explanation of the lifecycle of the periodical cicada that contains platonistic mathematical concepts and which is therefore not a purely causal evolutionary explanation, although he thinks it is nevertheless an example of a *genuine* platonistic

³⁶ Cf. Baker & Colyvan (2011).

explanation of an empirical phenomenon. This, if successful, would undermine the view that there are no genuine platonistic explanations of physical phenomena, and *a fortiori* the view that there are no genuine non-causal evolutionary explanations of physical phenomena. It would also bring the mapping account into question insofar as that account does not say anything about how platonistic concepts could explain empirical phenomena. I shall argue against this in what follows.

3.3 A Candidate for a Genuine Platonistic Explanation?

The piece of literature that has generated most of the recent discussion surrounding platonistic explanation is Alan Baker's paper 'Are There Genuine Mathematical Explanations of Physical Phenomena?' (2005).³⁷ Baker reinforces the need for such an example with a reflection on the state of the literature: "Colyvan has not come up with any unequivocal cases of mathematical explanation in science, and Melia has not given any non-question-begging grounds for thinking such explanations are impossible" (Baker 2005, p.229).³⁸ The subject of the example is the seven species of the genus *Magicicada* of the cicada family of insects, which possess a lifecycle which is a prime number of years long, viz. 13 or 17 years.³⁹ Periodical cicadas live underground for most of their lives at a depth of at least 30cm, and attach themselves to tree roots, from which they feed. Every 13 or 17 years, depending on species, they emerge in synchrony in order to mate and lay eggs.⁴⁰ The reason for this mass emergence is the biological phenomenon of 'predator saturation', whereby [cicada] "densities are so high that predators apparently eat their fill without

³⁷ When Baker says 'mathematical' in this paper, he usually means 'platonistic'. This is evident as his motivation for furnishing the cicada example is to support the indispensability argument for platonism.

³⁸ This refers to Colyvan (2001), (2002), where some possible cases of genuine mathematical explanation are presented, and Melia (2000), (2002) where both the indispensability argument, and the possibility of genuine mathematical explanation, are criticised.

³⁹ Not three species as Baker erroneously mentions – rather three species have a 17-year cycle (and are often found in the southern states), and four species have a 13-year cycle (and are often found in the northern states). Since all periodical cicadas are of the genus *Magicicada*, we may use the terms 'Magicicada' and 'periodical cicada' interchangeably.

⁴⁰ The most recent emergence is the summer of 2011.

significantly reducing the population” (Cooley & Marshal 2000). The reason why this mass emergence is prime-periodic is, as Baker shows, usually explained in terms of both biological and platonistic-mathematical facts.

Baker identifies five features of *Magicicada* lifecycle that biologists wish to explain, the first four of which receive straightforward biological explanations. However, the fifth, that the lifecycles of the cicadas are prime, is allegedly not so amenable to purely biological explanation. There are, Baker says, two different explanations that biologists have advocated (Baker 2005 p.230-231). The first of these is that *in an earlier period of their evolutionary history periodical cicadas evolved in response to being the prey of predators that unlike the cicada-eating creatures of contemporary America, were also periodical*. These predators would likely have low-number periods due to the fact that they would need to feed reasonably regularly, even with a slow metabolism, for unlike the periodical cicadas they could not simply subsist from a subterranean tree root. Prime-numbered lifecycles, it is argued, helped cicadas to avoid emerging at the same time as the predators. The second explanation is that *prime lifecycles enabled periodical cicadas to more successfully avoid mating with other cicada species with different periods* in an era in which there were more periodical cicada species than just the prime-numbered *Magicicada*. This meant that cicadas would avoid producing offspring with different lifecycle periods (a ‘hybrid’) whose divergent lifecycles would greatly reduce mating opportunities and increase predator exposure. I shall focus on the first of these in this section.

It is evident that if a cicada wished to minimise contact with periodical predators, the best way is to minimise the frequency of the intersection of their emergence with that of the predators, in mathematical terms, to possess a lifecycle which maximises the lowest common multiple (lcm) of the two periods. Baker explains that the “fundamental” concept in the platonistic-mathematical explanation of such a minimisation is not in fact the monadic property of primeness but rather the dyadic property of ‘coprimeness’. Two numbers are coprime iff their only common factor is ‘1’. A lemma of number theory is given that relates maximal lowest common multiple and coprimeness (ibid. p.232):

Lemma 1: the lcm of m and n is maximal iff m and n are coprime.

Thus if a cicada's lifecycle m and the predator's lifecycle n are coprime then the frequency of intersection of cicada and predator lifecycles will be minimal. Baker then shows that we can get from the coprimeness of the cicada/predator lifecycles to the primeness of the cicada lifecycle with an additional lemma of number theory:

Lemma 2: m is coprime with all n such that $(n < 2m \text{ and } n \neq m)$ iff m is prime.

That is, any number less than twice an arbitrary prime number that is not that prime number will be coprime with that prime number. So if a cicada has a prime lifecycle its lifecycle will be coprime with the lifecycle of the predator and that it will thus intersect minimally with the lifecycle of the predator.

Baker gives the entire explanation as follows (p.233, my emphasis), with both platonistic and non-platonistic components, claiming that "the purely mathematical component (2) is both essential to the overall explanation *and* genuinely explanatory in its own right":

1. Having a lifecycle period which minimises intersection with other periods is evolutionarily advantageous. (Biological Law)
2. Prime periods minimise intersection compared to non-prime periods. (Number-Theoretic Theorem)

1 and 2 imply 3:

3. Thus organisms with periodical lifecycles are likely to evolve prime periodical lifecycles. (Mixed Law)

4. Cicadas in ecosystem-type E are limited by biological constraints to periods ranging from 14-18 years (12-15 years). 3 and 4 imply:

5. Cicadas in ecosystem type E are likely to evolve 17 (13) year periods.

If primeness plays a genuine role in the explanation of the lifecycle then we can see why a philosopher might hold that we are committed to platonistic objects (prime numbers) and platonistic concepts (primeness as a property of numbers).

Although coprimeness and not primeness is doing most of the explaining, the primeness is important because a prime number is automatically coprime with a wide range of other numbers. But although prime periods p minimise intersection because they are coprime to all numbers $n < 2p$ that are not p , there are other non-prime numbers m which also minimise intersection relative to certain n , that is, composite numbers which are coprime with some n , e.g. 4 is coprime with 21. As not only prime numbers are coprime, primeness of a lifecycle is not essential in order for the lifecycle to minimise intersection with predators and there are instances where a non-prime lifecycle can be more advantageous than a prime lifecycle. The vast majority of predators will be non-periodical, and so will have a lifecycle of up to and including one year. Thus every time a periodical cicada emerges it will find itself in the company of non-periodical predators. But since such predators equally affect all cicadas of whatever lifecycle, prime or not, we may cease to consider them here, and need only consider periodical predators. Let us assume, with Baker, that the ecological conditions restrict northern-state cicadas to a lifecycle within the 14-18 year range. If the only periodical predators around have a three year lifecycle, then there is no reason for evolution to select a 17 year lifecycle over e.g. a 14 year lifecycle, as 14, as well as 17, is coprime with 3. Moreover, it is more evolutionarily advantageous for a cicada to have a lifecycle of 14 years, than 17 years, all predation being equal, since it will have more frequent opportunities to mate. Both will be vulnerable to non-periodical predators every time they emerge but in a given time-frame of e.g. fifty years, the 14 year cicada with its shorter lifecycle will be able to mate more frequently than the 17 year cicada and thus adapt/evolve more quickly. Another example reinforces this – if the periodical predators have lifecycles of 2 and

4 years, a 15 year cicada lifecycle is as coprime with 2 and 4 as 17 is, and again offers a mating advantage. So prime lifecycles do not offer any advantage in these cases.

It would thus only make sense for a cicada to evolve to be prime-periodical if there were many different predators with many different lifecycle periods themselves. For if the predators are 1, 2, 3 and 4-year periodical then a prime number lifecycle will evolve since it will be coprime with every possible predator lifecycle – but this is just to say that in the case that there are periodical predators *for all relevant possible lifecycles a periodical predator can occupy* then the prime number lifecycle will evolve. And if the focus is shifted from predation to hybridisation, the point still stands: yes a cicada may mate with cicadas of other periods and produce hybrids, and being coprime with these other cicada lifecycles offers an advantage in avoiding them. But if these other cicadas do not have periods of every possible relevant type then there is no guarantee that being prime is an advantage to a cicada lifecycle, and as with the example above, could in fact impede mating, since there will be shorter periods that will likely result in mating opportunities as safe, hybridisation-wise, as the prime periods, but which offer more frequent opportunities for mating. If a rational cicada got to choose its lifecycle behind a ‘veil of ignorance’, ignorant of actual predation and mating conditions, it would be a safe bet to choose a prime lifecycle. But although evolution does not work like this, and in fact progresses due to actual, and not merely possible, conditions that affect natural selection, it is still the case that if there are many cicadas and predators with a broad range of periods prime lifecycles will be evolutionary advantageous, and given the undeniable fact that the magicicada has a prime lifecycle it seems that must be what has happened, if evolutionary theory is at all true. The question is, is the use of the platonistic (number-theoretic) concept of primeness, or the prime numbers, in this explanation, essential to it? Some philosophers have argued that it is not, as shall I.

3.4 Arguments Against the *Cicada* Example

Helpfully for our purposes here, Baker, in a recent article (2009), categorises the various negative responses that have been made to his cicada example. I am going to focus on two of these that seem especially worth discussing: that the choices of mathematical objects and concepts in the explanation are arbitrary; and that the explanandum itself is not purely physical and thus begs the question against the impossibility of genuine platonistic explanations of physical phenomena. I argue that although these arguments are initially compelling, Baker's responses to both of these criticisms are adequate. To these therefore I add an objection of my own, that we don't need to appeal to primeness (or more importantly, coprimeness) as *platonistic* components in the genuine explanation of what is going on, since we can state primeness and coprimeness nominalistically. This shows that the role of the platonistic objects here need be nothing more than representational, a way to talk about empirical structures (of years for instance), but not essential, in principle, to explanations concerning them, and that platonistic concepts can be replaced with nominalistically acceptable concepts which can do the explaining just as well as the platonistic concepts.

3.4.1. Arbitrariness

The concern with respect to arbitrariness is that if something is arbitrary to an explanation of x it is not necessary to explain x , and cannot thus be part of the *genuine* explanation of x . It seems therefore that a well-supported accusation of arbitrariness is a worry to the believer in genuine platonistic explanations of physical phenomena. There are three possible charges of arbitrariness that can be levelled at the platonistic mathematics that is used in Baker's explanation (cf. Baker

2009), though I am concerned with only two here – object-level arbitrariness and concept-level arbitrariness.

Object-level arbitrariness is the claim that the specific platonistic (and indeed some of the physical) objects referred to in the explanation are arbitrary. For example instead of the 13 or 17 years that form prime periods, we could have discussed 676 or 884 months, neither of which is a prime number. Baker's answer to this charge is that the years *are* physically significant in this example given that cicada lifecycles must be governed at least in part with respect to the seasons. There is perhaps more to say here, but Baker's response is cogent, and so we can progress to more concerning criticisms.

Concept-level arbitrariness means that the *concepts* involved in the explanation are arbitrary, e.g. the very notions of 'prime' and 'coprime'. If there are explanations of the cicada example which have, as far as our current resources are concerned, an equal claim to be genuine and that do not invoke primeness, then primeness would seem to be arbitrary. This objection would seem to affect both platonistic and nominalistic statements of primeness. But can we give a good explanation that does not involve primeness? Juha Saatsi (2007) presents an idea for how we can avoid primeness, e.g. by utilising a more *specific* explanation, in the sense that what is important is that the periods are 13 and 17, not that the periods are prime. This would not of course demonstrate that prime numbers are arbitrary, but could demonstrate that the concept of primeness is arbitrary, at least to this explanation. How could we use 13 and 17 without explicitly using primeness? Saatsi suggests laying down sticks of 1 unit (and 2 units and 3 units...to e.g. 18 units) end to end and observing that concatenations of sticks of length 13 units or 17 units need to be longer than concatenations of sticks of up to 18 units in order to make concatenations of length equal to any other concatenations. This would convey that concatenations of 13 and 17 units are best for maximising the lowest common multiple, at least within a certain range, without explicitly invoking the concept of primeness. This objection is however vulnerable to the counter-objection that it may be incoherent to invoke prime numbers without invoking primeness itself, especially if an explanation of why 13 and 17 are best for maximising lcm is

requested. Granting that Baker's example way well be immune to charges of arbitrariness, both objectual and conceptual, let us progress to the second objection, that Baker's example is question-begging.

3.4.2 Begging the Question

Sorin Bangu (2008) believes he can undermine Baker's strategy by showing that what is being explained in the cicada example by appeal to number theory is an already partly platonistic phenomenon, and that the example therefore begs the question in favour of platonistic mathematics being a genuine part of the explanation. Specifically, the issue is that Baker's explanandum is not purely physical because the explanandum in question is 'periodic cicada lifecycles *are prime*'. Bangu contends that this explanandum clearly contains the platonistic concept of primeness, and that it is no surprise that the explanans of a platonistic-mathematics-containing explanandum has to involve platonistic mathematical facts. As Bangu points out, if the explanandum did not contain any platonistic mathematics there may be little need for the explanans to contain any either, leading us to the conclusion that the explanandum begs the question against the impossibility of genuine platonistic explanations of physical phenomena if those phenomena are described platonistically, and if they are not described platonistically there would seem to be no need to bring platonistic mathematics into their explanations. If the cicada example does beg the question in favour of what it is trying to establish, then it can't very well be accepted as showing that there are genuine platonistic explanations of empirical phenomena.

Baker agrees with Bangu that this argument has a certain plausibility. However, he does think that there is a way out. Baker's counter-argument runs as follows (Baker 2009, pp.620-621). We begin with two pieces of data: (1) the length (in years) of cicada species A's lifecycle is 13 and (2) the length (in years) of cicada species B's lifecycle is 17. This, explains Baker, is nominalistically acceptable (i.e. does not involve any of the platonistic mathematics at issue) since 13 and 17 can be paraphrased into purely logical terms in the usual way whereby we say 'there is a

thing₁, and another thing₂...and another thing₁₃ all with the property of being a year of a cicada's lifecycle, none of these things are the same, and if anything else has the property in question then it is one of the aforementioned things'. The need to interpret 13 and 17 as names for abstract mathematical objects has thus been dispensed with, and at present no question has been begged. Now, Baker needs to show that moving from 13 and 17 to primeness does not beg any questions. One way of doing this would be to provide a logical definition of primeness just as he was able to provide logical definitions of the numbers, but Baker is sceptical of this. At least, he agrees that this cannot be done with first-order logic, and, I presume because of the possibility that second-order logic may be set-theory in disguise, does not consider second-order formulations of the concept.⁴¹ So he needs another argument to show that we can move from 13 and 17 to primeness without begging any questions.⁴²

Baker explicitly gives such an argument (ibid. p.621-622). I add the actual argument below each premise in brackets:

- (i) Biological data, D
[species A has a lifecycle of 13 years]
- (ii) Tentative hypothesis H containing mathematical concepts
[A has a prime lifecycle]
- (iii) Explanation E of H which is also an explanation E* of D
[that periodical lifecycles are likely to be prime explains why A has a prime lifecycle – and why A has a 13 year lifecycle]
- (iv) E* is the best explanation of D

⁴¹ Baker also neglects to consider any possible plural-logic formulations of primeness, based on work such as Boolos (1984) and Hossack (2000). In this thesis my approach is to follow Rizza (2011) and treat years as unit-intervals of some sort and then talk about primeness nominalistically in terms of congruence of segments partitioned into such intervals. The plural logic approach is very interesting but due to space restrictions here I will have to examine it in future work.

⁴² It might be thought that Baker can rest content once a logical definition of 13 and 17 is provided, and ignore the concept of primeness altogether. Even if one were happy to proceed without such a definition (though as I said in connection with Saatsi's objection, such an endeavour may not even be coherent) it is the properties of prime numbers in general and not just the properties of numbers 13 and 17 that seems to explain the lifecycles of both the 13 and 17 year cicadas AND why cicadas would likely develop prime lifecycles. For the claim that 'periodical lifecycles are likely to be prime' does have the form of an explanation of (1) and (2) and also enables us to make predictions about other periodical creatures, namely that they are, biological constraints permitting, likely to evolve prime lifecycles.

- [that periodical lifecycles are likely to be prime is the best explanation of why A has a 13 year lifecycle]
- (v) So we should believe E* and therefore, E.
[so we should believe that periodical lifecycles are likely to be prime]
- (vi) But D and E together imply H
[the fact that species A has a lifecycle of 13 years together with the principles that periodical lifecycles are likely to be prime imply A has a prime lifecycle]
- (vii) So we ought to believe H
[so we ought to believe that A has a prime lifecycle]

The question of course is, does this ingenious combination of inference to the best explanation and ‘bootstrapping’ meet Bangu’s clear and concise objection? *Prima facie* one may well think that it does not – hesitancy could lie in the fact that Baker recommends that “from a philosophical perspective therefore we do not at this stage endorse [(1*) A’s lifecycle is prime or (2*) B’s lifecycle is prime] for fear of begging the question” (ibid. p.621). Rather we ‘tentatively advance’ (1*) and (2*). And it might be thought that it is not apparent how tentatively advancing that something is prime is any more nominalistically acceptable than endorsing outright. After all, if I tentatively open an email, the important thing is *that* I opened it, not how, and so it may be felt that this strategy is little more than equivocation on Baker’s part, for the purpose of masking the inadequacies in the cicada account and justifying Baker’s intuitions. But to believe this is to approach the issue of ‘tentative advancing’ with a lack of subtlety – if one goes carefully, and charitably, through Baker’s new argument, without construing ‘tentative advancing’ as just an adverbially modified advancing, but rather as a sort of hypothetical advancing, it is clear that no question is in fact being begged here. So Bangu’s objection is met and Baker, for now, is safe.

3.4.3 Dispensing with Platonistic Mathematics: An Objection to Baker

I agree that Baker’s example is a good one, but I deny that the existence of genuine platonistic explanations of physical phenomena follows from it. This is because I think that the number-theoretic explanation of the cicada lifecycle can be nominalised. Baker’s argument rests on the idea that to remove the number-

theoretic (and therefore platonist, since numbers are abstract mathematical objects) claims from the explanation of the periodical cicada lifecycle would severely impoverish it, that we want to keep the explanation so we had jolly well better keep the platonistic part of it. But if it is possible to nominalise the explanation then we can avoid the poverty of either having to make do with only specific causal stories peculiar to particular cicadas, or accepting that platonistic mathematics plays a genuinely explanatory but non-causal role.

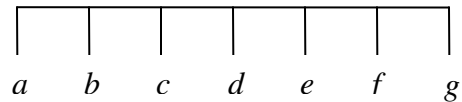
My strategy will be to show that we can give non-platonistic definitions of those concepts that possessed such unifying power in Baker's example, namely the concepts of primeness and coprimeness, and thus that the platonistic construal of these concepts is dispensable to the genuine explanation of the cicada lifecycle. If we can do so then the argument that there are genuine platonistic explanations of physical phenomena loses its force. Now, Baker explicitly says "...the number-theoretic notion of primeness plays a key role, and—despite the relative simplicity of the mathematics involved—no easy nominalistic paraphrases are available" (Baker 2009 p.619). No easy paraphrases perhaps, but certainly not *no* paraphrases. Indeed we can give, as I will shortly show, a nominalistic paraphrase.

The usual way of thinking about properties such as primeness is as a property of *numbers*. But we do not have to think like this. One alternative is a geometric interpretation: treat such properties as pertaining to *intervals*.⁴³ For instance, in the cicada case, divisibility will be a relation between time-intervals. I propose to follow Davide Rizza's approach in his paper 'Magicalcicada, Mathematical Explanation and Mathematical Realism' and nominalise the cicada example by using the geometric approach.

The basic units in the cicada example are years. Sequences of years are time-intervals, and these time-intervals themselves may be composed of smaller time-

⁴³ If we dislike this because we consider that geometry is too platonistic, Rizza says we can think of the various properties involved as empirical, and talk about congruence and divisibility etcetera as empirical relations among attribute-intervals (Rizza 2011 p.107f). Indeed I think there must be such properties and relations, and I discuss this briefly below. However these properties and relations are precisely what we want to represent, to abstract from, in order to facilitate counting, measuring, and the description of attributes etc. As such a more general language is required, and this is what the geometric approach (and of course the platonistic approach) provides.

intervals. For clarity I will refer to the larger intervals as sequences, and the individual year-long intervals as unit-intervals. Although natural numbers form an infinite ordinal sequence, properties such as primeness and operations such as multiplication do not depend on this infinity. For instance, 17 would be prime even if there were no numbers after 20 (in this case 40% of the numbers would be prime, so primeness would not be such a rare and interesting property!) The upshot of this is that we can refer to the primality of some sequences with respect to unit-intervals even when all of the sequences involved are finite. These sequences can be represented by a line segment split into congruent unit-intervals which represent years. The unit-intervals must be congruent because years are all of the same duration. This gives:



“One may simply say that a sequence X of consecutive intervals ‘divides’ a sequence [of consecutive intervals] Y if successive congruent copies of X determine an interval congruent to Y ” (Rizza 2011, p.106). Thus if $X = ab$ and if $Y = ag$, then X will divide Y , since if we juxtapose X to copies of itself, we will get a sequence congruent to Y . ‘Juxtaposition’ of a sequence with itself here refers to there being successive congruent copies of that sequence. So far so good, and we haven’t referred to any platonistic entities at all. What of the concepts of primeness and coprimeness? A sequence of unit-intervals X will be coprime with respect to another sequence of unit-intervals Y if the only sequence dividing them both is a sequence congruent to the unit-interval. Suppose X is ad and Y is ag (ag is a prime sequence). The only sequence that can be juxtaposed to copies of itself in order to give both ad and ag is the sequence ab , which is congruent to the unit-interval. If a sequence X is prime it will be coprime with a very broad range of other sequences Y , namely all those sequences Y which are shorter than any sequence Z where Z is congruent to X juxtaposed to a copy of X , and where Y is not identical to X . A sequence X will be

prime when the only sequence that can divide X is the unit-interval and X . Thus nominalistic versions of primeness and coprimeness have been defined, and the need to give platonistic formulations of these concepts has been obviated. Since X being a prime sequence minimises the range of sequences which can divide both X and Y , the greatest common divisor of X and Y within the above mentioned range will be the unit-interval, so the range of possible divisors will be reduced to just that interval.

Suppes (and Rizza) also show that we can prove the existence of a representation function from the empirical relational structure $\langle A, \succeq_4 \rangle$, where A is a finite sequence of consecutive and congruent unit-intervals, to the numerical relational structure $\langle \mathbb{N}, \succeq_4 \rangle$. The empirical comparison relation is $ab \succeq_4 cd$, where \succeq_4 is the quaternary relation ‘interval ab is longer than interval cd ’. A structure containing this relation is known as a finite equal-interval difference structure. The numerical comparison relation \succeq_4 is the quaternary relation $m - l < o - n$. Rizza (2011, p.107) explains that the appropriate axioms for proving the required representation theorem are to be found in Suppes (1972) paper ‘Finite Equal-Interval Measurement Structures’. This theorem (with the vocabulary altered to reflect that used in this thesis) is:

“Let $\langle A, \succeq_4 \rangle$ be a finite, equally-spaced difference structure. Then there exists a real-valued function ϕ on A such that for every a, b, c, d in A , $ab \succeq_4 cd$ iff $\phi(a) - \phi(b) \succeq_4 \phi(c) - \phi(d)$.” (Suppes 1972, p.50).

Finite, equally-spaced difference structures (including the above sequence of years) are those which satisfy the following axioms: (1) \succeq_4 imposes a weak order; (2) if $ab \succeq_4 cd$ then $ac \succeq_4 bd$; (3) if $ab \succeq_4 cd$ then $dc \succeq_4 ba$; (4) If aJb and cJd then $ab \sim cd$. Axiom 4 expresses the equal-spacing assumption, the congruence of the intervals, and its predicate J is defined as obtaining iff $a > b$ and for all c in A , if $a > c$ then either $b \sim c$ or $b > c$. (Suppes 1972 p.50). If the reader is interested in the proof of the theorem they may consult Suppes 1972, pp. 56-58.

We thus see that although numbers can be invoked to give a familiar way of treating the primeness and coprimeness of sequences of years, no numbers in fact have to be appealed to at all in order to keep the explanation. This means it is possible to express when the intersection of the lifecycles in years of those cicadas and predators will be minimised without using abstract mathematical objects, that we can talk about minimised periods of intersection without reference to the lowest common multiples or greatest common divisors of pairs of *numbers*. For if two sequences of years are coprime, the intersection of predators and cicadas possessing those sequences as lifecycles will be minimised, since by definition the year-long unit-interval is the smallest unit of time in the sequences. Platonistic mathematics may be a useful way of talking about empirical attributes, and this usefulness extends to the making of very particular descriptions and predictions, but it is a tool only, and there is no reason, despite Baker's attempt, to think it a feature of any genuine explanations of physical phenomena, least of all the lifecycles of the periodical cicada. For the dispensability of the platonistic explanation shows that it is not a genuine explanation, regardless of what our attitude towards of platonistic explanation is.

Now this may be though superficially similar to Saatsi's attempt exposited above (as mentioned in Baker 2009) to show that the references to prime numbers in the cicada example are not essential, by instead talking about sticks laid end to end, and seeing that what is important is that they are 13 and 17 units long, not that they are prime. But this is not so. Firstly I agree that the primeness is an essential part of the explanation, but I deny that we have to think about this platonistically, or draw any inferences about the existence of abstract mathematical objects from it. Secondly, the geometric approach outlined above can easily be adapted to a variety of situations, and thus has a generalness that Saatsi's example lacks. Numbers may be a good way to talk about common structural properties that different systems possess, but they are by no means the only way to do so: the geometric approach utilised above also enables us to speak generally about structural properties of systems of objects by talking about properties of sequences of intervals. It may be objected that even if these intervals are not as nominalistically objectionable as

numbers they do appear to have an abstract character, especially insofar as it does not seem to be the properties of any *particular* sequence that is doing the explaining. Moreover there may be different sequences doing the explaining in different cases, whereby we have properties of lots of different sequences explaining diverse situations, and have gained concreteness at the price of the unity, the unity that the number-theoretic approach provides in explanations of different explananda. One possible response to this is to talk about resemblances between these different sequences, along the lines of one standard nominalist response to the metaphysical realist about universals and types. Unfortunately I do not have space to pursue this further here, but it is certainly an avenue for future research.

One quick point. It may be objected that I earlier endorsed only causal explanations, and that was reason I wanted to do away with platonistic explanations of empirical phenomena. So it may be asked exactly what trading the platonistic evolutionary explanation for the nominalistic one has gained me. After all, it is not as if I am suggesting that the geometric representation of the year-intervals has a causal power the platonistic definition lacks. Whether platonistic or nominalistic it seems primeness and coprimeness are still doing the explaining. My response to this is to take Rizza up on his suggestion that we treat the property of primeness and relations of divisibility, coprimeness etc as primitive empirical properties and relations. Thus we can see primeness as a genuine property of time-sequences, and coprimeness as a genuine relation between them. The reason the cicadas survive is because of these empirical relations between their life cycles, relations whose properties are best understood by ascending to a greater level of abstraction. The unit-intervals of these empirical sequences are what are *represented* by the platonistic objects, and the properties and relations of these sequences are what are represented by the platonistic concepts of primeness and co-primeness (*mutatis mutandis* in the geometric approach by the nominalistic versions of these objects and concepts, viz. intervals and interval properties). The beauty of the nominalistic approach is that it offers a way of representing empirical structures that does not use platonistic objects, and is thus available to the philosopher who wishes to nominalise the mapping account – a strategy pursued in chapter six.

A similar view to my representational conception of applied mathematics (though not directly concerning the cicada example, which came later) is endorsed by Joseph Melia in his paper ‘Weaseling Away the Indispensability Argument’ (2000). Melia argues that abstract mathematical objects are not explanatory in their own right, but merely *index* elements of the empirical situation which themselves do the explanatory work, e.g. the fact a is $7/11$ metres from b merely indexes a certain distance relation, namely that a and b are the distance apart that they are. That is to say, Melia is claiming in effect that the platonistic mathematics here is purely representational. I give the quote in full. It concerns a mathematical theory T_2 :

The fact that T_2 is capable of generating infinitely many distance predicates using only a few primitives does not entail that the distance relations expressed by these predicates are themselves not primitive, irreducible, relations. Indeed, in this particular case, although T_2 expresses the fact that a is $7/11$ metres from b by using a three place predicate relating a and b to the number $7/11$, nobody thinks that this fact holds in virtue of some three-place relation connecting a b and the number $7/11$. Rather the numbers are used merely to index different distance relations, each real number corresponding to a different distance. (Melia 2000, p.473).

This reference to indices is a clear gesture at a mapping account of applicability, where to say that numbers index distances is to say that they measure distances, that a number represents a distance. Baker concedes that “even if [Melia – and *a fortiori* myself] were to concede [that primeness is ineliminable from the cicada example], Melia could still maintain that the [platonistic] mathematics is not genuinely explanatory in its own right but rather is a non-explanatory component of a larger explanation” (Baker 2009, p.622). And this is exactly what I have said above, and in chapter two – we may indispensably need some sort of mathematical structures and concepts (platonistic or nominalistic) for measuring empirical phenomena and for both arriving at and stating scientific theories, but the mapping account holds that the use of mathematics here is purely representational, that it is, as Baker puts it, a non-explanatory component of a larger, empirical, theory.

I have argued above that there are no such things as genuinely platonistic explanations of empirical phenomena, for in no way is the platonistic mathematics essential to the explanation, which is a necessary criterion for genuineness to be

attributed. We saw that the most notable case of a philosopher trying to locate an uncontroversial example of such an explanation (drawn from biology not physics) has been Baker's cicada example. I then evaluated these arguments, conceding to Baker that none of them seemed entirely convincing, before presenting my own argument against him, along the lines that we can give a nominalistic version of the objects and concepts involved in terms of sequences of time-intervals and their properties and relations. Since these are available there is no reason to think platonistic mathematics a genuine part of the explanation. It may be objected that this does not show the dispensability of platonistic explanations of empirical phenomena, only dispensability in this single case.⁴⁴ But I believe that enough has been said to make cast significant doubt on the possibility of finding other necessary examples of such explanations. Until (*per impossible*) such an explanation is found I will persist in my claim that there are no such explanations, and that any account of the applicability of mathematics must focus on the representational aspects of mathematics only.⁴⁵ At the very least both parties to the debate must agree with Colyvan that "[t]he debate over platonism and nominalism would be genuinely advanced by a better understanding of explanation – especially those explanations that have mathematics playing the leading role" (Colyvan 2010, p.304). The next chapter, chapter four, will examine in detail how it is possible to give nominalistic versions of more complicated platonistic-mathematics-containing empirical

⁴⁴ Recall that Baker does mention other cases, e.g. Colyvan's example of antipodal points, but he expresses reservations about its explanatoriness (Baker 2005 p.226-7). As regards Colyvan's examples of the bending of light and Minkowski space-time, Baker is concerned that geometrical explanations may not be viewed as platonistic – they could be viewed as based on facts about physical space. To he clearly takes his cicada example to be the only case given his paper that he feels unequivocally supports his argument.

⁴⁵ It has been pointed out to me that Lyon and Colyvan (2008) claim such an example with the explanatory power of phase spaces in the explanation of galactic stability. They argue that phase-space theories cannot be nominalised, and that even if they can much explanatory power is lost in the nominalisation. They say "...as Malament points out, it seems highly unlikely that Field can provide a nominalist account for the structure of phase spaces. Or at least nothing in Field's treatment of space-time indicates how phase spaces would be nominalised" (p.240). This is certainly a claim I would need to address in future work, but I would observe here that Lyon and Colyvan have not shown for *certain* such a nominalisation is impossible, and the fact a nominalisation does not follow easily from Field's treatment of space time is to a degree irrelevant here, as I have not claimed Field's approach is the only viable strategy towards nominalisation. Clearly further work needs to be done in order to buttress the notion that mathematics has only a representational role in applications.

theories, further undermining the view that platonistic mathematics is essential to any explanation of any empirical phenomenon.

Chapter 4

*Future discussions of this
area must take up where
Field leaves off.*

– Michael Friedman (1981)

Eliminating Mathematics from Natural Science

The purposes of this chapter are twofold. Firstly a positive proposal is given for showing how it is that we can state scientific theories in such a way that platonistic mathematics does not appear as part of those theories. This is essential, since although the previous chapter argued that there are no genuine platonistic explanations of empirical phenomena, it showed positively how we could dispense with mathematics in a only a fairly simple case. Secondly, this chapter shows in detail how we need only assume that the applicability of mathematics consists in the ability of mathematics to represent physical phenomena in a certain way, by giving an account of a scientific theory (Newtonian gravitational theory) in which the mathematics involved is purely extrinsic to, that is, has only a representational role in, the theory. This is achieved by showing that there is a nominalistic, intrinsic version of the aforementioned theory, which is equivalent to the platonistic version, as there is an isomorphism between the nominalistic and platonistic structures involved. These two aims are realised chiefly through an engagement with and exposition of Hartry Field's monograph, *Science Without Numbers*. In the final section I consider the possibility of giving a similar treatment of Quantum Mechanics.

4.1. Chapter Introduction

The question of whether or not platonistic mathematics is indispensable to science is central to contemporary philosophy of mathematics because so many discussions about the plausibility of platonism have directly concerned the issue of indispensability. As I have said before, this thesis is primarily concerned with the applicability of mathematics rather than the plausibility of platonism *per se*, although I do address the platonism/nominalism debate in chapter six, and said a little at the end of the previous chapter. The focus of that chapter was not on indispensability as an argument for platonism but rather with genuinely platonistic explanations of empirical phenomena, the main argument concerning which was given by Baker, and the purpose of my response was to justify the representational character of applied mathematics. Thus any strike against platonism *vis à vis* the undermining of Baker's attempt to salvage the indispensability argument has been entirely accidental. At this stage I am agnostic about platonism, though opposed to it having an indispensable role in explanations of empirical phenomena.

I have argued against Baker that his evolutionary example was not a genuine platonistic explanation of an empirical phenomenon because the platonistic mathematics was dispensable to it. I did this by showing that the platonistic mathematics used in Baker's example could be formulated in a nominalistically acceptable way. In this chapter I shall utilise Field's account of how to nominalise physical laws to demonstrate in detail how platonistic mathematics can be dispensed with in physical as well as biological explanations – specifically I look at Newtonian gravitational theory. Baker could grant dispensability in this case, holding that the platonistic mathematics in Newtonian gravitational theory does not have the sort of explanatory role he claimed for platonistic mathematics in his evolutionary example, but rather a descriptive role that can be dispensed with. Not all philosophers who think that platonistic mathematics is essential to scientific explanations would agree with this descriptive-role attribution however, since Newtonian gravitational theory is stated in a very platonistic way, with

quantification over an array of abstract mathematical objects. Such philosophers would include those confirmational holists who are sceptical of the Fieldian programme and fans of the indispensability argument.

If it can be shown that platonistic mathematics is dispensable in both some evolutionary and some classical mechanical cases, and if there is no good reason not to think the success of these examples can be reproduced, then this bodes well for the prospect of showing that such mathematics is inessential to scientific theories in general. Since the evolutionary example was defused, so to speak, in the last chapter, I shall address the classical mechanical case in this chapter, as well as taking a look at the possible extension of the methods used in the classical mechanical case to Quantum Mechanics, which has been advocated by Mark Balaguer. I go into significant detail in both these cases to make it clear what is being accomplished and how these programmes actually work. The purposes of this enterprise is to see in much greater detail, and for an actual physical theory, how it is that platonistic mathematics is dispensable, and that the role of such mathematics here is a purely representational one, explicable, as I argued in chapter two, in terms of mappings between empirical and mathematical structures.

4.2. Field's Nominalisation of Newtonian Gravitational Theory

In 1980 Hartry Field published a monograph containing a nominalist philosophy of mathematics, *Science Without Numbers*. Although Field intended his book to establish the plausibility of nominalism, it contains much material relevant to the philosopher who is agnostic about platonism but who is investigating the applicability of mathematics, and it was one of the first detailed philosophical treatments of applicability. Field regarded the indispensability argument as the best argument for mathematical platonism, stating...

...there is one and only one serious argument for the existence of [abstract] mathematical entities and this is the Quinean argument that we need to postulate such entities in order to carry out ordinary inferences about the physical world and in order to do science. (Field 1980 p.5)

The truth or otherwise of the substantive claim in this quotation – that the Quinean indispensability argument is the only serious argument for platonism – is not relevant to the considerations in this chapter, although I discuss this argument briefly in chapter six. We need only recognise that it was the indispensability argument which provoked Field to seek to demonstrate that platonistic mathematics, though very useful for scientists, is not in fact indispensable to science. Whilst Field has attempted to show that this mathematics is (in principle) dispensable to science, he acknowledges that for scientists to directly adopt the nominalistic way of doing science without platonistic mathematics would be very difficult indeed, and Field admitted that he does “not of course claim that nominalistic concepts are anywhere near as convenient to work [with] in solving problems or performing computations: for these purposes the usual numerical [i.e. platonistic] apparatus is a practical necessity” (Field 1980 p.91). I shall now turn to the details of how Field’s programme actually works.

4.2.1 The Background to the Fieldian Programme

Although Field believes that platonistic statements are not literally true and that abstract mathematical objects and structures are useful fictions he does not have to reject or denounce the practice of platonistic mathematics along with all its utility, since he establishes the conservativeness of platonistic mathematics over nominalist science, thereby licensing the purely instrumental use of platonistic mathematics by the nominalistically inclined scientist without the possibility of any nominalist consequences that were not already implicit in the nominalist premises. I shall not present the details of conservativeness in this chapter, since (a) it is largely irrelevant to the development of non-platonistic science, and (b) the issue is

addressed in detail in chapter six, where I am more directly concerned with the existence of abstract mathematical objects and the plausibility of Field's programme as a *sui generis* nominalist programme rather than just as a means to produce non-platonistic versions of scientific theories.

Field explains that one benefit of a non-platonistic formulation is that "it would be illuminating if we could...[have] an explanation that did not invoke functions to extrinsic...entities" (Field 1980 p.43). Such an explanation would not make references to either arbitrary coordinate systems, or to arbitrary units for scalar attributes such as temperature and mass, or indeed to any abstract mathematical objects at all. Chapter two of this thesis explained *how* it is that mathematics can be applied, given its representational role, and made it clear *why* mathematics is applied, namely it makes things a great deal simpler. Below we see in detail how it is that platonistic mathematics makes things a great deal simpler in a particular case, namely Newtonian gravitational theory. What we see however is that just because one theory makes another one easier to state does not mean that it is essential to that other theory. It may have been unintentional, but Field, in the course of trying to nominalise science, actually shines much light on this very topic and indeed the mapping account itself, by showing in several instances how it is that platonistic mathematics can usefully represent empirical phenomena and assist in the development and statement of empirical theories despite being extrinsic to the subject matter of those very theories.

4.2.2. Nominalising Newtonian Space-Time and Magnitudes of Scalar Attributes.

Before showing how science can be nominalised, Field establishes some results concerning the conservativeness of platonistic mathematics over nominalist theories, that platonistic mathematics is not essential to the derivation of the nominalist consequences of a nominalist theory. At any rate he establishes that *if* it is possible to produce a nominalistic scientific theory then platonistic mathematics

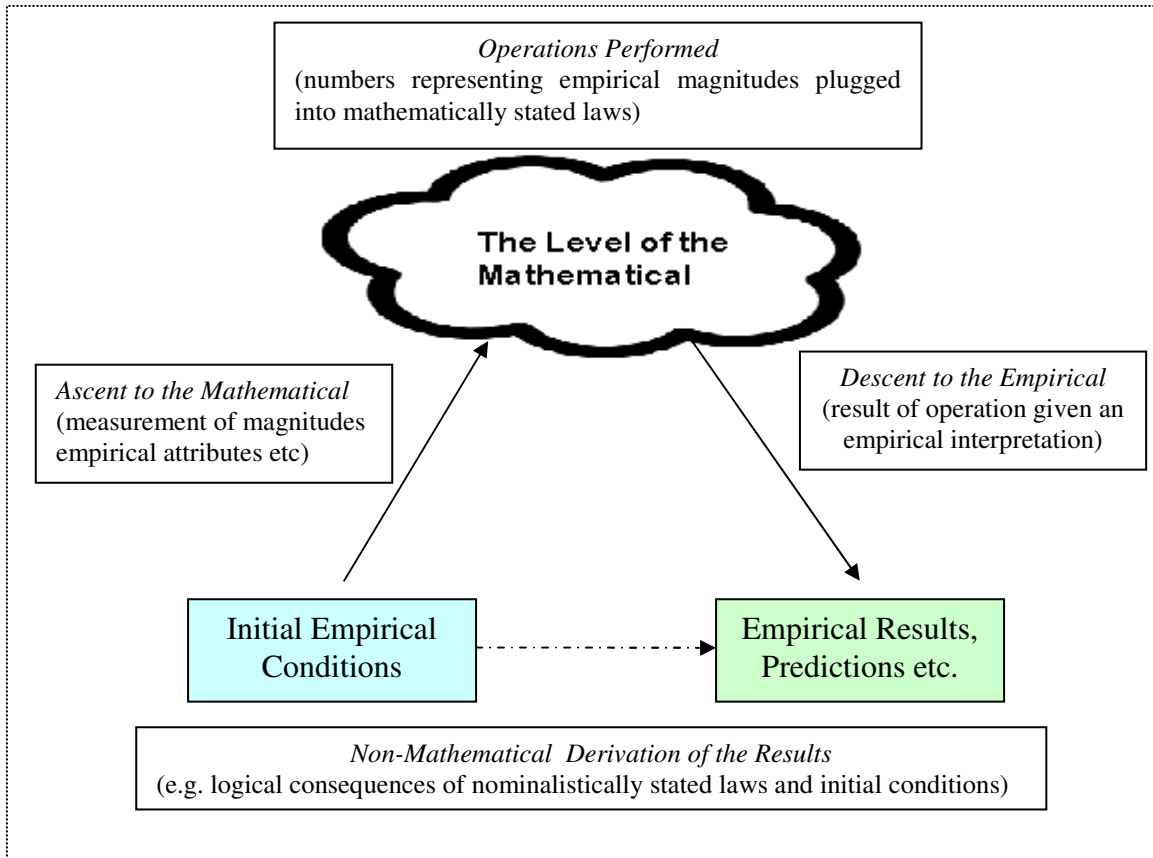
will be a conservative extension of that theory. It is not until the nominalisation of Newtonian gravitational theory is complete that conservativeness becomes not just hypothetical, but a reality. For to show not just that conservativeness is a possibility in the case of scientific theories but that there is some such (nominalised) scientific theory such that platonistic mathematics is a conservative extension of it, such a theory has to be constructed. Because, as I stated above, the fact of conservativeness itself is not directly relevant to the specifics of how to produce a nominalised version of a scientific theory I omit discussion of it here but will take it up again in chapter six.

The nominalisation of Newtonian gravitational theory proceeds through the proof of representation theorems concerning isomorphisms between platonistic relational structures (in many instances the reals with addition and a comparison relation) and empirical relational structures (in this instance space-time points and various scalar attributes, with concatenation and a comparison relation). Using nominalistic predicates built out of primitive relations such as spatio-temporal betweenness, which are represented by platonistic predicates such as 'x is greater than y but less than z', Field arrives at nominalist definitions of various platonistic concepts. These platonistic definitions hold if, and only if, the nominalistic definitions do, a fact demonstrated by the existence of isomorphisms between the platonistic and empirical structures. Eventually enough concepts are defined that it is possible to give a nominalist statement of Newton's gravitational laws. This section (4.2.2) will consider the proof of the necessary representation theorems, the next section (4.2.3) will consider the nominalist definitions of the various concepts that are essential for stating Newtonian gravitational theory nominalistically.

In order to nominalise Newtonian space-time, Field uses the resources of measurement theory, normally construed as a branch of platonistic mathematics. It might be objected that Field has no place using such mathematics to establish his programme. Field's response is that this is not an issue – he is showing the platonist, in platonist terms, why platonistic mathematics is dispensable to science, and is not "trying to...provide a positive argument for nominalism but to undercut the only available argument for platonism". (Field 1980 p.4). The undercutting of the

indispensability argument is fine as far as my account of applicability in this thesis is concerned, since I could perfectly well be a platonist about mathematics and a nominalist about science, *so long as the use of platonist mathematics in science was not the source of my belief in platonism*. If this caveat is satisfied, I could help myself to platonistic mathematics for any legitimate purpose, including showing that platonistic mathematics is not essential for nominalistic science. For if a person believes in platonistic mathematics it does not follow that he believes in genuine platonistic explanations of physical phenomena, for instance. As an aside it may be wondered how the nominalist can account for the applicability of mathematics, a phenomenon undeniable whatever our views on what mathematics is and what it is about. The nominalist will thus need to provide an account of applicability, a fact underscored by Field himself who states that the problem of applicability is “the really fundamental one” (Field 1980, p.vii). It is clear that if there is a way of nominalising the mapping account, and thus measurement theory, in a non-circular manner then the nominalists can, in the manner of Sir Francis Drake, have the luxury of being able to explain applicability and defeat [some of] the platonists too. But that’s enough about metaphysics for now – I shall return to issues of platonism and nominalism in a later chapter.

Let us now recall some of the content of chapter two. Measurement theory explains exactly how it is that a mathematical structure can represent an empirical structure, justifying our ascent to the mathematical level, our performance of operations on the mathematical structure, and our subsequent descent to the level of the empirical with an empirically relevant answer. This is shown by the following illustration:



Remember, a central part of measurement theory consists in the proof of representation and uniqueness theorems for the measurement of magnitudes of empirical attributes that are ordered in a certain way. Representation theorems establish a homomorphism ϕ between an ordered empirical relational structure and a (frequently) numerical relational structure such that for any two objects a and b of the domain of the empirical structure and an empirical comparison relation $>$ concerning a given empirical attribute, $a > b$ iff $\phi(a) > \phi(b)$, which means that the numerical structure can then be used to *measure* the magnitude of that attribute of a and b which is being considered. If the empirical domain were rods, the attribute length, and the numerical domain the real numbers, then the relevant representation theorem would show a homomorphism between lengths of the rods and the real numbers.⁴⁶ Further conditions introduce an operation of concatenation,

⁴⁶ It was shown in chapter two that it makes sense in fact to view the empirical domain as a set of equivalence classes of (actual and possible) empirical objects with a given attribute if we want a full

show that concatenation is additive, and ensure that the measurement is sensitive to the units involved.

Representation theorems thus prove that one collection of entities can represent another collection if the stipulated conditions are met. One of these collections will be the platonistic objects that are the real numbers in very many cases, ranging from the extensive measurement of lengths to some of the laws of classical mechanics. The other collection will consist of empirical objects and one, or some, of their attributes. In the case of Newtonian space-time Field envisages the empirical objects in question as ‘space-time points and regions of them’, an ontology motivated by Field’s substantivalist view of physical space. The debate over whether such entities are or aren’t nominalistically acceptable is relevant here, since if such space-time points and regions are not so acceptable then they cannot very well be used in a version of our scientific theories that does not quantify over abstract objects. However Field claims that “according to such theories [that take the notion of a field seriously] space-time points are causal agents in the same sense that physical objects are” (Field 1980 p.114, note 23). I direct the reader to Field’s 1984 paper ‘Can we Dispense with Space-Time?’ for a persuasive account of the matter, and must now let it rest.

Having granted space-time points as empirical objects, how are the appropriate representation theorems proved, and what do they say? The first representation and uniqueness theorems that need to be proved are those concerning the representation of spatio-temporal distance (where distance is a distinctly platonistic notion since it is given in numerical terms) by betweenness relations, and relations constructed out of betweenness relations, on space-time points.⁴⁷ The proofs are not given due to lack of space, but any reader familiar with Krantz *et al* (1971) will see that these theorems are uncontroversial enough.

isomorphism between real numbers and the empirical structure, since not every possible magnitude of e.g. length, for instance, will be instantiated.

⁴⁷ The material in the box is distilled from chapter six of Field (1980), and much of it is quoted verbatim.

The Representation and Uniqueness Theorems for Newtonian Space-Time

(R_{ST}) – The empirical relational structure $\langle N, \text{Simul}, \text{Bet}, \text{S-Cong} \rangle$ satisfies the Szczerba-Tarski axioms *if and only if* there is an isomorphism ϕ from N to R^4 such that

- (a) $\forall x, y [x \text{ Simul } y \leftrightarrow \phi_4(x) = \phi_4(y)]$
- (b) $\forall x, y, z [y \text{ Bet } xz \leftrightarrow d_\phi(x, y) + d_\phi(y, z) = d_\phi(x, z)]$
- (c) $\forall x, y, z, w [xy \text{ S-Cong } zw \leftrightarrow ((\phi_4(x) = \phi_4(y)) \ \& \ (\phi_4(z) = \phi_4(w)) \ \& \ (d_\phi(x, y) = d_\phi(z, w)))]$

Where $d_\phi(x, y)$ is defined as:

$$d_\phi(x, y) = \sqrt{\sum_{i=1}^4 (\phi_i(x) - \phi_i(y))^2}$$

$\phi_i(x)$ is the i th component of the quadruple $\phi_4(x)$, where each i corresponds to a dimension of space-time. Each i th difference is squared to ensure there are only positive differences – the square-rooting of the whole sequence is for the purposes of undoing the effects of the squaring once the positive distance has been obtained.

N is the collection of space-time points, Simul is a (binary) simultaneity relation on space-time points, ‘ x is simultaneous with y ’. Bet is a (ternary) betweenness relation on space-time points, ‘ y is between x and z ’. S-Cong is (quaternary) spatial congruence relation on space-time points, x and y are congruent to z and w .

(U_{ST}) - given any model of the axiom system and any two functions ϕ and ϕ' whose domain is the domain of the model: if ϕ meets the conditions of the representation theorem (i.e. of (R_{ST})), then ϕ' meets those conditions iff it has the form $(T \circ \phi)$; where T is a generalized Galilean transformation of R^3 , i.e. a transformation that can be obtained by some combination of shift of origin, reflection, rotation of axes, and multiplication of all coordinates by a positive constant (and where ‘ \circ ’ indicates functional composition).

According to the representation theorem R_{ST} therefore, anything we could say about space-time using numerical distances and platonistic distance functions, we can say without using any such distance functions, and *a fortiori*, any numbers, at all. Moreover, the above-mentioned Szczerba and Tarski (1964) helpfully provide a set of geometrical axioms for affine geometry (a type of geometry that subsumes under its umbrella the geometry of Newtonian space) which use only the betweenness relation, and are therefore nominalistically acceptable alternatives to axioms that are usually stated numerically. In case the reader should be concerned about the

adequacy of these axioms, Szczerba and Tarski assure us that “...we easily obtain an adequate axiomatization by proceeding exactly as in the metric case...In this way we arrive, e.g., at the following axioms system...” (Szczerba and Tarski 1964, p.168).⁴⁸

This axiom system is:

The Szczerba-Tarski Axioms

Note: unless otherwise specified all variables are implicitly universally bound.

A1. Identity Axiom: $yBxx \supset x = y$

A2. Transitivity Axiom: $(yBxz \ \& \ zByu \ \& \ y \neq z) \supset yBxu$

A3. Connectivity Axiom: $(yBxz \ \& \ yBxu \ \& \ x \neq y) \supset (zByu \vee uByz)$

A4. Extension Axiom: $(\exists x)[yBxz \ \& \ (x \neq y)]$

A5. Pasch’s Axiom: $(\exists v)[(tBxu \ \& \ uByz) \supset (vBxy \ \& \ tBzv)]$

A6. Desargue’s Axiom:

$xBtx' \ \& \ yBty' \ \& \ zBtz' \ \& \ yBxu \ \& \ y'Bx'u' \ \& \ zBxv \ \& \ z'Bx'v \ \& \ zByw \ \& \ z'By'w \ \& \ \sim(xBty) \ \& \ \sim(yBxt) \ \& \ \sim(tByx) \ \& \ \sim(yBtz) \ \& \ \sim(zByt) \ \& \ \sim(tBzy) \ \& \ \sim(zBtx) \ \& \ \sim(xBzt) \ \& \ \sim(tBxz) \ \& \ ((x \neq x') \supset vBuw)$

A7. Lower Dimension Axiom: $(\exists x)(\exists y)(\exists z) [\sim(yBxz) \ \& \ \sim(zByx) \ \& \ \sim(xBzy)]$

A8. Upper Dimension Axiom:

$(\exists u) [\{uByz \ \& \ [uBxt \vee tBxu \vee xBtu]\} \vee \{uBxy \ \& \ [uBzt \vee zBtu]\} \vee \{uBxz \ \& \ [uByt \vee yBtu]\}]$

A9. Elementary Continuity Axiom Schema

$(v)(w)\{(\exists z)[(\phi \ \& \ \psi) \supset xBzy] \supset (\exists u)[(\phi \ \& \ \psi) \supset uBxy]\}$ where ϕ stands for any formula in which the variables x, v, w, \dots , but neither y nor z nor u may occur free and similarly for ψ , with x and y interchanged.

⁴⁸ Note that I have rendered the axioms somewhat differently to Szczerba and Tarski to keep them in line with the style used by Field, thus ‘ x is between y and z ’ is ‘ x Bet yz ’ and not ‘ $B(yxz)$ ’.

E. Euclid's Axiom

$(\exists v, w) [\{uBxt \ \& \ uByz \ \& \ (x \neq u)\} \supset \{yBxv \ \& \ zBxw \ \& \ tBvw\}]$

Since the Szczerba-Tarski axioms describe the geometry of Newtonian space-time, and because points in Newtonian space-time satisfy these axioms in the same way that quadruples of real numbers satisfy the numerical version of these axioms, we can conclude that Newtonian space-time can be represented nominalistically.

Much remains to be done however in order to nominalise the entirety of Newtonian gravitational theory. For instance a nominalistic treatment of scalar quantities is needed, since Newtonian gravitational theory talks not only about distance relations between Newtonian space-time points, but also about comparisons between magnitudes of scalar attributes that regions of such space-time points have, specifically the mass at a region, or the gravitational potential at a region. The earlier representation theorem R_{ST} has taken care of distance, but has omitted any mention of scalar attributes. Each scalar attribute needs its own representation theorem stating that the magnitudes of the attribute and some empirical comparison relation on these magnitudes maps on to the real numbers with the greater-than relation. Fortunately the form of the theorem is the same in the case of each *scalar* attribute, and so a theorem-template can be used, where the required attribute can just be inserted where needed. The representation (ψ) and uniqueness theorems for mass are (Field 1980 pp.56-57, *mutatis mutandis*):

The Representation and Uniqueness Theorems for Scalar Quantities

(R_{Mass}) – The empirical relational structure $\langle N, \text{Scal-Bet}, \text{Scal-Cong} \rangle$ satisfies the axioms describing the scalar attribute in question (perhaps the axioms of extensive measurement considered earlier in chapter two) *if and only if* there is a homomorphism ψ from N to a connected subset of R such that

- (a)** $\forall x, y, z [y \text{ Scal-Bet}_{\mathcal{A}} xz \leftrightarrow \text{either } \psi(x) \leq \psi(y) \leq \psi(z) \text{ or } \psi(z) \leq \psi(y) \leq \psi(x)]$
- (b)** $\forall x, y, z, w [xy \text{ Scal-Cong } zw \leftrightarrow |\psi(x) - \psi(y)| = |\psi(z) - \psi(w)|]$

(U_{Mass}) - Given any model of the axiom system and any two functions ψ and ψ' whose domain is the domain of the model: if ψ meets the conditions of R_{Mass} then ψ' meets these conditions iff it has the form $(T \circ \psi)$. Where Mass-Less is not used as a primitive, T is a linear transformation of the reals (a function of the form $ax + b$). If Mass-Less is used as a primitive, T is a positive linear transformation (a linear transformation $a > 0$).

Thus, anything we can say about magnitude of a scalar attribute using numbers, we can say using space-time points and regions, and empirical relations on them. It may be thought that there is no homomorphism here, because betweenness and congruence do not map onto the 'greater or equal to' relation. However 'having the same or greater mass', \geq , does map onto the 'greater or equal to' relation, and betweenness and congruence are constructed out of these in a way that preserves the mapping. So we would expect that since $a \geq b$ iff $\psi(a) \geq \psi(b)$, a is between b and c iff $b \geq a$ and $a \geq c$, or $c \geq a$ and $a \geq b$ if, that is if, $\psi(b) \geq \psi(a)$ and $\psi(a) \geq \psi(c)$ or $\psi(c) \geq \psi(a)$ and $\psi(a) \geq \psi(b)$, and the same for congruence. And these are exactly the definitions Field gave above, so the mapping holds.

4.2.3. Nominalising the Gravitational Theory and its Constituent Concepts

To give a thorough account of Field's nominalisation of Newtonian gravitational theory would be an extremely large undertaking that would take us far afield from the explanation of applicability with which this thesis is primarily concerned, and would moreover likely require such vast quotations from Field's work that its originality would be highly diluted. This is because pages 61-91 of *Science without Numbers* consist primarily of sketches of, and dense attempts at producing, nominalist definitions of platonist concepts that appear in our physical theories, definitions which for obvious reasons it is not easy to reproduce in one's own words. I nevertheless do want to discuss Field's nominalisation, and present some of the nominalistic definitions he gives of some platonistic concepts. After this has been done I shall summarise what Field has achieved, and look at how it is related to the mapping account.

We saw above that the platonistic description of Newtonian space-time and numerical magnitudes of arbitrary scalar attributes of points and regions in that space-time can be replaced by talk of nominalistically acceptable structures. What we need next is a nominalisation of the *laws* concerning these attributes, since it is the business of physical science to state physical laws. Field notes: "...physical laws governing a scalar [attribute] like temperature or gravitational potential are often expressed as laws about a scalar function T , mapping quadruples of real numbers into real numbers" (Field 1980 p.59), in other words, mapping spatio-temporal coordinates into platonistic values of measurement. Since ϕ takes points of space-time to quadruples of reals, and ψ takes points of space-time to reals, the composition of ϕ and ψ , namely $(\psi \circ \phi^{-1})$, will take quadruples of reals to reals. $(\psi \circ \phi^{-1})$ is thus T , the subject matter of the platonistically-stated physical laws which we wish to nominalise.

With this in mind, we stipulate that there is a joint axiom system (JAS) of the Szczerba-Tarski axioms and the empirical axioms governing the attribute in question such that if the empirical structure satisfies the JAS then there will exist representation functions ψ and ϕ . In a simple case the axioms governing the scalar attribute will be minimally the axioms for extensive measurement, but Field considers a more complex case where they are the axioms for gravitational potential and mass-density – in which case there will be $T_1 = (\psi \circ \phi^{-1})$ and $T_2 = (\rho \circ \phi^{-1})$ containing the scalar representation functions, ψ and ρ , and the gravitational law will describe the relation of T_1 and T_2 . The JAS will of course involve the various primitive predicates (spatio-temporal and scalar) which have been defined for the set N of space-time points. Once the JAS is established and the representation theorems are proven then we know that we can dispense with the platonistic structure in favour of the empirical structure, since we can talk about space-time points, and their relations, rather than real numbers and relations of numbers.

The question is, in all cases will we be able to do this? Field suggests that we can by actually doing it in the case of Newtonian gravitational theory. So far all that has been nominalised is talk of Newtonian space-time and scalar attributes

themselves. We have not yet shown how to nominalise the statement of the relations of these attributes to each other, that is, how to nominalise scientific laws. Representing space-time and scalar attributes was a fairly simple task, but to explain their relations, that is, to state the natural laws, requires quite a lot more apparatus, and as we saw at the end of chapter two the exact nature of this relationship is frequently quite complicated, though it will be attempted shortly in the case of gravitational theory. Usually in science, especially physics, a key part of the apparatus for expressing laws involves differential equations. Is Field able to nominalise differential equations as easily as he nominalised the numerical representation of Newtonian space-time, and scalar magnitudes? It might be thought that the real numbers can easily be nominalised so long as there is some sort of physical continuum to represent them, but that differential calculus is a very different matter. The reason for this view is that it is not necessarily clear what nominalistically acceptable relations of space-time points and their attributes would correspond to the platonistic relation described by the calculus. I shall try to show in what follows that this is a mistaken view, though a good deal of this will be a sketch. We need to establish that some nominalist version of a law is equivalent to the platonist statement, that N iff P . One question is: does the nominalist structure need predicates besides those that can be constructed out of empirical comparison relations on space-time points and their attributes in order to get the job done? Field argues that, at least in the case of Newtonian gravitational theory, it does not.

There are a lot of nominalistic versions of platonist concepts to be constructed, and as I said above, I cannot realistically give them all here. Field attempts nominalist definitions, or at least sketches of such definitions, for all of the following platonistic concepts: continuity; products and ratios (signed and unsigned); derivatives (first- and higher-order); Laplaceans; Poisson's Equation; inner products; gradients; differentiation of vector fields; and a Law of Motion. Before I progress any further, note that Field is not nominalising Newton's work as it appears in his *Principia Mathematica*, but rather a more modern formulation of his gravitational theory. Field explains:

By the Newtonian theory of gravitation I mean the theory of motion for an arbitrary particle, assuming that the only forces acting on the particle are gravitational forces. Given the space-time framework, which I have already shown how to handle nominalistically, the Newtonian theory of gravitation can be stated in two laws: a field equation governing a certain scalar field (the gravitational potential) and an equation of motion. The field equation is Poisson's equation [and the equation of motion is Newton's]. (Field 1980 p.78)

Thus, to nominalise Newtonian gravitational theory it suffices to nominalise Poisson's equation and the relevant law of motion. We know that if the platonist structure (numbers, platonistic operations and relations) is sufficient for expressing the two laws then the nominalist structure (space time points and empirical predicates, operations and relations) will likewise be sufficient, so long as there is an isomorphism between the two structures. What Field needs to show is that there is such an isomorphism. He has already shown there is an isomorphism between some less complex platonist and nominalist structures, and now he has to show that this isomorphism obtains between the structures we are interested in, as concerns the Newtonian gravitational theory.

Poisson's Equation. This equation is a partial differential equation that is a field equation, meaning that it describes how a fundamental force (in this case, gravity) interacts with matter. A field is a physical property associated with each point in space-time. Poisson's equation in its general form is $\Delta\phi = f$, or specifically for gravity, $\Delta\phi = 4\pi G\rho$. The left hand side is the Laplace operator (which Field discusses, *ibid* p.76.) operating on a function representing gravitational potential at a point in a space. The right hand side of the equation is product of number 4, pi, the universal gravitational constant (i.e. three constants) and the mass-density of the point. Thus "at any point the Laplacean of the gravitational potential is proportional to the mass-density at that point..." (Field 1980 p.78). Field points out that we can restate Poisson's equation thus, where x and y are points, and rho is the symbol for mass-density. (I have stated this in logical notation for clarity):

$$(x)(y)[(\Delta\phi_x = 0 \text{ iff } \rho_x = 0) \ \& \ ((y \neq x \ \& \ \rho_x \neq 0 \ \& \ \rho_y \neq 0) \supset ((\Delta\phi_x :: \Delta\phi_y) = (\rho_x :: \rho_y)))]$$

This formula then is what Field needs to nominalise. It is evidently not nominalistically acceptable as it stands, because the Laplaceans and mass-densities are given numerically. To facilitate showing that this is equivalent to some nominalistic definition this we need to break the formula down into its constituent conjuncts: (Field pp. 79-80)

- (a) at any point the Laplacean of the gravitational potential is zero iff the mass-density of that point is zero
- (b) at any two points at which the mass density is not zero, the ratio of the Laplaceans of the gravitational potential is equal to the ratio of the mass-densities.

We can then reconstruct (a) and (b) using the nominalistic versions of platonist concepts that Field has defined earlier in his monograph, e.g. the nominalist definition of ratio. Mass-density is slightly more complicated than attributes such as mass or length, as it is a derived and not a fundamental attribute, but that does not affect the substance of the argument. As Field himself notes, we can talk about comparisons of ratios of mass-density differences with ratios of gravitational potential differences in a manner similar to his earlier nominalistic treatment of ratios of other quantities. We shall see an example of some of these concepts after a brief look at the second half of the gravitational theory, the law of motion.

The Law of Motion. As will be familiar to anyone with even the slightest knowledge of physics, there are various statements of the law of the motion of an arbitrary particle. The statement Field selects involves the notion of a tangent to a trajectory of such a point-particle (or trajectory-like region of space-time). “A tangent to a trajectory T at point z is a straight line S through z such that the directional derivative of the spatial separation between T and S with respect to any vector exists and is zero at T . The tangent to T is unique if it exists...” (Field 1980 p.89), and a trajectory is differentiable at z if it has a tangent at z and this tangent is not purely spatial. The preliminary part of the law of motion consists in the claim that ‘the trajectory of any point particle is both trajectory-like and differentiable’. I shall quote extensively from Field now:

The main part of the law of motion requires that we compare the accelerations of points on the same or different trajectories with the gradients of the gravitational potential at those points. Let T and T' be any trajectories and let z and z' be any points on them. Let S and S' be the tangents to T and T' at z and z' respectively and let y and y' be points on S and S' such that \overrightarrow{zy} and $\overrightarrow{z'y'}$ are temporally congruent and have the same temporal orientation. The law of motion is then simply that there is a positive real number k such that:

- (1) the second directional derivative of the spatial separation of S from T at z with respect to \overrightarrow{zy} taken twice is k times the gradient of the gravitational potential at z
- (2) the second directional derivative of the spatial separation of S' from T' at z' with respect to $\overrightarrow{z'y'}$ taken twice is k times the gradient of the gravitational potential at z' .

Since Field has defined nominalist versions of the concepts, such as derivative, used in this statement of the law of motion, it is, as with Poisson's equation, a relatively simple matter to produce a nominalist statement of this law. The interest here is not so much that the law can be nominalised, but rather with the ingenuity that Field uses to nominalise the component concepts in the law. I briefly want to outline three of these concepts, Field's treatment of continuity; ratio and product; and his treatment of derivative or rate-of-change. As we shall see, and as with the nominalisation of Newtonian space-time above, Field uses ever more complex predicates built out of relations on space-time points and scalar attributes, and combinations of them, to define nominalist versions of the various concepts required.

Continuity [of a scalar attribute with respect to a region of space-time]. Given that the scalar attribute is represented by scalar function T , this continuity can be expressed platonistically by saying that T is a continuous function. But how do we say this nominalistically? Field's answer is to introduce the notion of 'basicness' of space-time regions. *Space-time region R is spatio-temporally basic* when R contains only those points z that are strictly spatio-temporally between x and y . *Space-time region R is scalar-basic* when R contains only those points z such that the magnitude of z with respect to a given scalar attribute is strictly between the magnitudes of x and y with respect to that attribute. We can then assert a biconditional which gives

both the platonistic and nominalistic content of a region of space-time points being continuous with respect to some scalar attribute:

(a) for all space-time points x , for any scalar-basic region containing x there is a spatio-temporally basic sub-region containing x iff T is continuous at $\phi(x)$ '.

The left hand side of this biconditional (which Field refers to as the claim 'CONT') is purely nominalistic since it uses only the betweenness relation, logical vocabulary, and predicates built up logically from space-time points, their empirical attributes, and the betweenness relation.

Ratios. In many cases we will want to say that one interval, or product of intervals is less, or more, than another interval or product. In other words, we will want to talk about the ratio of the relevant intervals. In a very simple case we will just want to say one spatio-temporal interval is less than another, which can obviously be stated nominalistically in terms of betweenness and congruence, since if one interval is less than another the intervals will not be congruent. But in the majority of cases we actually want to talk about the relative magnitudes of scalar attributes that the points in the intervals in question possess. Now suppose we want to say that the product of one spatio-temporal interval $x_1 x_2$ with a scalar interval $y_1 y_2$ is less than the product of the spatio-temporal interval $u_1 u_2$ with a scalar interval $v_1 v_2$. We would, says Field, need to prove the following biconditional, where ST-SCAL is a nominalistic predicate that does the job (we will see below how that predicate is defined):

(b) $|x_1 x_2 y_1 y_2|_{\text{ST-SCAL}} |u_1 u_2 v_1 v_2|$ iff
 $[d_\phi(x_1, x_2) |\psi(y_1) - \psi(y_2)| < d_\phi(u_1, u_2) |\psi(v_1) - \psi(v_2)|],$

where d_ϕ is the platonistic spatio-temporal distance function $\sqrt{\sum_{i=1}^4 (\phi_i(x_1) - \phi_i(x_2))^2}$, and $\phi_i(x)$ is the i th component of the quadruple $\phi(x)$. I shall state this biconditional in English as well, to aid comprehension:

(b*) The product of one spatio-temporal interval $x_1 x_2$ with a scalar interval $y_1 y_2$ is nominalistically-speaking less than the product of the spatio-temporal interval $u_1 u_2$ with a scalar interval $v_1 v_2$ *if and only if* the product of (the magnitude of the difference of the real numbers measuring the scalar magnitude across $y_1 y_2$) with the distance of the first spatio-temporal interval is less than the product of (the magnitude of the difference of the real numbers measuring the scalar magnitude across $v_1 v_2$) with the distance of the second spatio-temporal interval.

Whether or not this biconditional is acceptable depends on the nominalist predicate in question – if the predicate does the job it should, then the biconditional is fine, if not, then it isn't. Currently ST-SCAL is just a placeholder for a definition not yet given. How is the predicate defined? Field gives the nominalist definition of the octadic ST-SCAL predicate, with temperature as the scalar attribute in question, as:

$u_1 \neq u_2$ and $v_1 \not\approx_{\text{Scal}} v_2$ and if $x_1 \neq x_2$ and $y_1 \not\approx_{\text{Scal}} y_2$ then $\exists R_{\text{ST}} \exists R_{\text{Temp}} [R_{\text{ST}}$ is a spatio-temporally equally spaced region and R_{Temp} is a temperaturely equally spaced region; x_1 and x_2 are in R_{ST} and y_1 and y_2 are in R_{Temp} ; there are a, b in R_{ST} such that u_1 and u_2 are spatio-temporally between a and b , and there are c, d in R_{Temp} such that v_1 and v_2 temperaturely between c and d ; there are *just as many points* of R_{ST} that are spatio-temporally between x_1 and x_2 as there are points of R_{Temp} that are temperaturely between v_1 and v_2 , and *there are fewer points* of R_{Temp} that are temperaturely between y_1 and y_2 than there are points of R_{ST} that are spatio-temporally between u_1 and u_2 . (Ibid. p.66)

Clearly, this uses yet more predicates, some which have not been defined, namely being 'spatio-temporally equally spaced' and 'scalarly equally spaced'. The above definition is very complex, but it should be clear to the reader that equal spacing will probably be definable using the more primitive notion of congruence outlined in section 4.2.2, and utilised in the nominalistic definition of primeness in the previous chapter. And indeed this is broadly correct – equal-spacedness is defined as a sort of congruence relation:

'spatio-temporally equally spaced region'	'scalarly equally spaced region'
is a region R all of whose points lie on a single line such that for every point x of R which lies strictly spatio-temporally between two points of R, there are points y and z of R such that (a) exactly one point x of R is strictly spatio-temporally between y and z and (b) $xy \text{ P-cong } xz$. [P-Congruence is congruence along parallel lines]. (Ibid. p.65)	is a region R such that for every point x of R which lies strictly scalarly between two points of R, there are points y and z of R such that (a) exactly one point x of R is strictly scalarly between y and z and (b) $xy \text{ Scal-cong } xz$. (Ibid. p.66)

These two predicates also involve further predicates such as parallel congruence and scalar congruence, but these do not present any further conceptual difficulties and so there is no need to define them here. Additionally the cardinality relations in the definition of ST-SCAL, 'just as many' and 'fewer than', present no problems, since they can be given nominalistically acceptable definitions in terms of betweenness and congruence, or if we prefer, purely logically. So the ratios in question can be given a nominalistically acceptable definition.

Derivatives. We have seen, and perhaps been surprised by, the way in which some platonistic concepts such as ratio can be given a nominalistic definition. But the reader may feel that differentiation is in a category of its own. How can derivatives be defined nominalistically, specifically the partial derivatives that are so ubiquitous in the law-like statements of modern physics? This must be possible however, since derivatives are rates of change, and there is no reason to think rate-of-change is not plausibly an empirical concept. Field's solution is to look for *comparisons* of directional derivatives with scalar intervals rather than to say outright that such and such a partial derivative has such and such a value. I shall give the biconditional outright and then explain how Field defines it:

(c) $D(x, a_1, a_2, b_1, b_2)$ *iff* the directional derivative of $T (= \psi \circ \phi^1)$ with respect to the vector $\phi(a_2) - \phi(a_1)$ exists at $\phi(x)$ and has a value there equal to $\psi(b_2) - \psi(b_1)$.

As with ST-SCAL above, the predicate $D(x, a_1, a_2, b_1, b_2)$ clearly needs to be defined if this biconditional is to be satisfactory. The directional derivative of T with respect to the vector $\phi(a_2) - \phi(a_1)$ exists at $\phi(x)$ and has a value there equal to $\psi(b_2) - \psi(b_1)$ iff and only if $D(x, a_1, a_2, b_1, b_2)$ obtains. The somewhat long-winded definition of $D(x, a_1, a_2, b_1, b_2)$ is:

$(b_1 \approx_{\text{Scal}} b_2 \ \& \ \exists b \{ \exists c \exists d (b \text{ [strictly] SCAL-BET } cd) \ \& \ (a_1 = a_2 \supset b \approx_{\text{Scal}} b) \ \& \ [a_1 \neq a_2 \supset (\forall c \forall d \text{ (if } b \text{ [strictly] SCAL-BET } cd \supset \exists y \exists z \text{ } yz \text{ PAR } a_1 a_2 \ \& \ x \text{ [strictly] ST-BET } yz \ \& \ \forall t (t \neq x \ \& \ t \text{ [strictly] ST-BET } yz \supset (a_1 a_2 xt) \text{ E-BET}_{\text{ST, SCAL}} (xtbc)(xtbd)))]]) \vee (b_1 \not\approx_{\text{Scal}} b_2 \ \& \ \exists a_3 \exists b_3 [a_3 \text{ ST-BET } a_1 a_2 \ \& \ a_1 a_3 \text{ P-CONG } a_3 a_2 \ \& \ b_1 b_3 \text{ SCAL-CONG } b_3 b_2 \ \& \ \{ \exists c \exists d (b_2 \text{ [strictly] SCAL-BET } cd) \ \& \ (a_1 = a_2 \supset b_1 \approx_{\text{Scal}} b_3) \ \& \ [a_1 \neq a_2 \supset (\forall c \forall d \text{ (if } b_3 \text{ [strictly] SCAL-BET } cd \supset \exists y \exists z \text{ } yz \text{ PAR } a_1 a_2 \ \& \ x \text{ [strictly] ST-BET } yz \ \& \ \forall t (t \neq x \ \& \ t \text{ [strictly] ST-BET } yz \supset (a_1 a_2 xt) \text{ E-BET}_{\text{ST, SCAL}} (xtb_1c)(xtb_1d)))]]) \}])$ (Field 1980 pp.70-72 – I have altered Field's style and inserted all the formulae Field only names for clarity and completeness).

It is evident that the terminology here is all nominalistically acceptable since it only contains space-time points, empirical attributes, logical vocabulary, and predicates ultimately built out of empirical comparison relations. For the second (or higher) derivative “we merely need to express the result of taking a first derivative, by means of betweenness and congruence predicates, and apply the whole process again” (Field 1980 p.74).

The rest of Field's construction proceeds similarly – Laplaceans are defined in terms of derivatives, and Poisson's equation in terms of Laplaceans. Likewise, the nominalist definitions of ratio and vector enable nominalist concepts of inner products, gradients and vectors, and eventually, the law of motion. I hope sections 4.2.2 – 4.2.3 have been sufficient to make it plausible that Field's programme achieves nominalistic success, at least with respect to Newtonian gravitational theory. I now want to discuss the issue of whether, the nominalistic acceptableness of Field's programme notwithstanding, it offers a genuine explanation of the motion of a particle under gravitational forces in Newtonian space-time.

4.2.4. Field's Nominalism and the Mapping Account

Before I discuss how Field's version of Newtonian gravitational theory relates to the mapping account outlined in chapter two, I think it will be useful to summarise Field's treatment of Newtonian gravitational theory. Indeed Field himself gives a good summary at the end of chapter eight of *Science Without Numbers*, which will assist me. Firstly nominalistic axioms for Newtonian space-time (box 2 above, the modified Szczerba-Tarski axioms) were given, whilst those for gravitational potential and mass-density were fairly clear given the treatment of extensive attributes in chapter two of this thesis. These axioms together formed a nominalistic Joint Axiom System, or JAS, which was satisfied by the domain of points in Newtonian space-time. The satisfaction of the JAS by the empirical domain implied the existence of several representation functions (boxes 1 and 3 above) stating that there is an isomorphism ϕ from space-time points to quadruples of real numbers, representing points in Newtonian space-time, and two homomorphisms ψ and ρ from space-time points to \mathbb{R} , which represented the magnitudes of the scalar attributes of gravitational potential and mass-density respectively of the points and regions. It was seen that the composition T of these representation functions took the quadruples of real numbers representing the points to the appropriate real numbers representing the magnitudes of the attributes in question, and that although physical laws normally concerned T , it was possible to dispense with T and talk about space-time points, their attributes, and relations on them instead, thereby avoiding any talk about real numbers.

Further concepts (namely Poisson's equation and the law of motion) were constructed from space-time points and regions and empirical comparison operations on them. No predicates were used which were not constructed out of clearly-defined nominalistically acceptable primitives, and no magnitudes of any empirical attributes were invoked which we were not sure could be, on the basis of our earlier discussion of representation theorems, represented by the reals. Thus we see that given the isomorphisms between the nominalistic and platonistic

structures, it was possible to state the laws governing the relations of the attributes of gravitational potential and mass-density, nominalistically.

Let us agree for argument's sake that this programme works. One concern is that the platonistic, i.e. classical, formulation of the theory is the explanatory one, and that all Field has done is to provide nominalistic surrogates for platonistic concepts, surrogates which are formally equivalent but in fact not really explanatory. However this is a foolish objection and that even if it is in practice impossible to do science the Fieldian way, this in no way reflects badly on the nominalist. The nominalistic formulation is no less explanatory than the platonistic one insofar as it manages to pick out the requisite empirical objects, properties and relations, and indeed has the advantage that it does not need to quantify over abstract objects and relations to do so. However a final quote from Field is appropriate here:

I do not of course claim that the nominalist concepts are anywhere near as convenient to work in solving problems or performing computations: for these purposes, the usual numerical apparatus is a practical necessity. But it is a necessity that the nominalist has no need to forgo: he can treat the apparatus in the way suggested earlier in the book, i.e. as a useful instrument for making deductions from the nominalistic system that is ultimately of interest; an instrument which yields no conclusions not obtainable without it, but which yields them more easily (Field 1980 p.91).

Of course, the question of what is the best nominalistic formulation of Newtonian gravitational theory is an open one: there may be a version other than Field's which is perhaps simpler or has greater unificatory power or some such. It is nevertheless evident that any nominalistic theory will have to account for spatio-temporal continuity, ratios between magnitudes of empirical attributes, and the rate of change of one property with respect to another. These are not inherently platonistic concepts, but rather part of the furniture of the universe: platonistic mathematics merely provides a very useful and intuitive way of talking about them.

I can now finally turn to one of the key questions raised at the beginning of this section, namely the relation between Field's nominalisation of Newtonian gravitational theory and the mapping account. Recall that the mapping account says that mathematics can be usefully applied because mathematical structures represent empirical structures in such a way that we can perform operations on

mathematical structures and arrive at an empirically relevant answer that could nevertheless (in principle) have been obtained without the help of the mathematics. It is plainly the case that Field's nominalisation is a perfect example of this. If I want to solve a problem in classical mechanics about the effect of gravitational forces on a point-particle, it is a very simple matter to obtain some numerical values for the variables in the relevant equations, plug them in, and get an (empirically correct i.e. empirically verified) answer.

But as Field shows, it is also possible to get that result without using any platonistic mathematics whatsoever, by using the (more complicated) nominalistic version of the theory as given above. So this is a perfect example of the mapping account at work, since it shows in detail both the nominalistic and the platonistic routes to the empirical answer (e.g. the left and right-hand components respectively of the biconditionals in Field's theory). Thus Field's theory comes across as an instance of the mapping account I outlined in chapter two, although if the nominalist wishes to make the theory totally nominalistic, e.g. to remove references to abstract mathematical objects from even the proofs of the representation theorems, he will of course have some extra work to do. This is the focus of part of chapter six. However before progressing onto chapters five and six, I want to take a look at one other attempt to nominalise a scientific theory, namely Mark Balaguer's sketch of a nominalisation of Quantum Mechanics, the most abstract of all empirical theories.

4.3. Balaguer's Nominalisation of Quantum Mechanics.

The purpose of this section is really just to reinforce that the dispensability of platonistic mathematics to science isn't just a fluke that applies only to Newtonian gravitational, and similar, theories – that the role of platonistic mathematics in science is a purely representational one across the board. The question that will be on the lips of philosophers will surely be 'can the Fieldian approach be extended to other physical theories?'. Field holds that "...it would be routine to extend the nominalistic treatment of gravitational theory to other theories with a similar

format, e.g. special relativistic electromagnetic theory” (Field 1980 p.42). But what about theories that do not have a similar format, such as General Relativity (GR) and Quantum Mechanics (QM)? Some philosophers, such as David Malament (in his 1982 review of *Science without Numbers*) are sceptical about how far Field’s programme can be extended.

One worry is that although Field manages to nominalise many parts of platonistic mathematics, including the differential calculus, more sophisticated areas of that mathematics may resist nominalisation. Field himself was reasonably optimistic about the prospect of extending his programme to cover General Relativity:

...I *believe* that without too much trouble all the mathematical developments [that we have just considered] could be generalised to a space-time with a more general sort of affine structure than that considered here...such as the space-time of general relativity” (Field 1980, p.64).

Can the same be said of Quantum Mechanics? Malament’s objection concerns the possibly abstract status of even the non-mathematical objects found in Quantum Mechanics – for if we construe QM as a theory which determines a set of Hilbert spaces, as is common, we would need to prove that subspaces of some Hilbert spaces merely represent purely empirical objects and their relations, and can be dispensed with in favour of these. An obvious candidate for the subject matter which the Hilbert spaces represent would be *propositions* or *eventualities*. The problem here is that propositions and eventualities may be at least as nominalistically unacceptable as Hilbert spaces are. Mark Balaguer believes he can provide a nominalistic reconstruction of QM that meets Malament’s objection, utilising *propensities*.

It is important to note that unlike Field, Balaguer does not provide a nominalistic reconstruction of the *laws* of Quantum Mechanics, e.g. the Schrödinger equation, but he believes that this can be done in the same manner that Field nominalises the laws of Newtonian gravitational theory. He does however try to provide a nominalist reconstruction of what Quantum Mechanics is about, similar to

what Field did with space-time points and Newtonian gravitational theory. The foundation of Balaguer's approach is that "quantum probability statements are about *physically real* propensities of quantum systems" (Balaguer 1996 p.217, my emphasis). Balaguer does make a small effort to defend propensities as physically real, attempting to refute what he takes to be the main form of Malament's worry, that "propensities are properties, and properties are abstract objects" (ibid. p.224). Balaguer's response is that we can deal with propensities the same way we deal with empirical attributes like length, namely by introducing a comparison relation like 'longer-than', in this case some sort of x has-a-greater-propensity-than y relation. Balaguer is correct that we are able to speak of empirical attributes in such a way that they don't refer to any sort of abstract objects, but as yet he has not shown that propensities are among such empirical attributes. If they are in fact abstract then evidently nothing nominalistic has been achieved. Balaguer's response is that propensities are causally efficacious, which rules out their being platonistic, abstract, objects. The problem still remains that propensities can only be causally efficacious if they actually exist in the relevant sense, and Balaguer has not, in his brief treatment, really and unambiguously shown us what a propensity is, that it is an empirical thing, which would be required in a fully worked out Balaguer-style nominalisation of Quantum Mechanics. However for the sake of the example I am content to grant Balaguer the empirical reality of propensities – what is more interesting is his actual strategy for how the nominalisation should take place.

The primary platonistic claim is that quantum states are functions from quantum events to probabilities, where a quantum event (A, Δ) is a measurement of an observable A that yields a result in some Borel set Δ of real numbers.⁴⁹ (I shall quote liberally from Balaguer 1996 for the next few hundred words, so for ease of presentation please assume that the technical material is from that source unless otherwise indicated). This quantum event will then determine a propensity property r of a quantum system to yield a value in Δ for a measurement of A (Ibid.

⁴⁹ Borel sets are used because they are required in certain parts of the theory of probability and measurement theory that I do not need to go into here. Suffice to say that roughly speaking "Borel sets are the sets that can be constructed from open or closed sets by repeatedly taking countable unions and intersections". (source – wolfram.mathworld.com)

p.218). Each quantum state q is then associated with a set $S(P)$ of such propensities.⁵⁰ The central claim made by Balaguer here is that from the set $S(P)$ a nominalistic orthomodular lattice $L(P)$ can be constructed which is homomorphic to the orthomodular lattice $L(H)$, constructed from the set $S(H)$ of closed subspaces of a Hilbert space H in which the given observables are represented (ibid p.218). If this homomorphism obtains then $L(P)$ can represent $L(H)$. The justification of this claim is a transitive one: $L(P)$ is homomorphic to $L(E)$, the lattice built out of the set of quantum events associated with given observables, and $L(E)$ is homomorphic to $L(H)$, ergo, $L(P)$ is homomorphic to $L(H)$. This depends on three claims, namely (a) that $L(P)$ is homomorphic to $L(E)$, (b) that $L(E)$ is homomorphic to $L(H)$, and (c) that homomorphism is preserved under transitivity, since if one structure can map another, and that one can map a third, the first can map the third, as they will all have the relevant structural features in common. What about the former two claims (a) and (b)? Earlier in his paper Balaguer defends the claim that $L(E)$ is homomorphic to $L(H)$:

we can define lattice-theoretic predicates on $S(H)$ and $S(E)$ and thereby construct (non-distributive) orthomodular lattices out of these two sets which are isomorphic to each other. We can call these orthomodular lattices $L(H)$ and $L(P)$ respectively. (Balaguer 1996 p.216).

He does not provide the construction explicitly, but for the purposes of the exposition I will assume that this claim is an accurate one and that (b) is unproblematic, since the reason that Hilbert spaces are used at all is for their representative capabilities, and platonistic mathematics is a rich source of such structures.

What of (a)? Once again, Balaguer does not formally prove a homomorphism between $L(P)$ and $L(E)$, but he does give an informal two-part argument for the truth of his claim. The first part is to show a homomorphism exists between the domains of $L(P)$ and $L(E)$ for any $L(P)$ - $L(E)$ pair. The second part involves demonstrating the existence of nominalistic versions of the lattice-theoretic

⁵⁰ Balaguer uses ψ to denote quantum states, but I am keen to avoid confusion with the representation function ψ used above to denote the representation of a scalar attribute.

predicates and operators. The first part proceeds by showing a homomorphism obtains between the domains of $S(P)$ and $S(E)$, since if their domains are homomorphic then so are the domains of $L(P)$ and $L(E)$. We have already said that $S(E)$ is a set of quantum events. Each quantum state q determines a probability function p_q from quantum events in $S(E)$ to real numbers (probabilities). Balaguer's suggestion here is that if we fix the quantum state of the system to a particular state, then each quantum event will be associated with a propensity, and that it is very easy to see how each propensity can be associated with a quantum event. That is, each member of $S(E)$ will be associated with a member of $S(P)$. The second part is a little more complex.

Just as Field needed to construct nominalistic predicates, so too must Balaguer. At this stage Balaguer needs fewer predicates than Field, because he has not gone into the details of nominalising those parts of platonistic mathematics and science beyond those which Field has already nominalised, and because he does not explicitly state the laws of Quantum Mechanics. However Balaguer must define platonistic predicates of inclusion and orthocomplement for both $S(H)$ and $S(E)$ – thus we have ‘ x is subspace-included in y ’ ($x \leq_H y$), ‘the subspace-orthocomplement of x ’ for $S(H)$, ‘ x is event-included in y ’ ($x \leq_E y$) and ‘the event-orthocomplement of x ’ for $S(E)$. Balaguer's method then is to construct nominalistic versions of the inclusion and orthocomplement predicates for $S(P)$, although he only gives the procedure for constructing propensity-inclusion in any detail. We can represent propensity-inclusion as ($x \leq_P y$). Balaguer's suggestion is to “lift the definition of \leq_P directly off the definition of \leq_E ” (Balaguer 1996 p.220). Since $(A, \Delta) \leq_E (A', \Delta')$ iff for every quantum state q associated with the given $L(E)$, $p_q(A, \Delta) \leq p_q(A', \Delta')$, then

(A, Δ, r) \leq_P (A', Δ', r') iff it is a law of nature that every quantum system has a propensity to have a value in Δ for a measurement of the observable A which is weaker than, or equal in strength with, its propensity to have a value in Δ' for a measurement of the observable A' . (Balaguer 1996 p.220)

Thus Balaguer feels that he has shown that $L(P)$ and $L(E)$ are homomorphic – although a representation theorem has not been proved – and thus how a

considerable part of QM can be nominalised. It is clear that if propensities are acceptable nominalistic entities then Balaguer has shown how part of Quantum Mechanics can be represented nominalistically. Just as the bulk of Field's work concerned the nominalisation of the laws of Newtonian gravitational theory, so Balaguer's work stands in need of expansion, as he has said, to cover the laws of Quantum Mechanics. This would be a very large and complex undertaking indeed, and there is no space here to consider how this might be done. We must rest content with the belief that it is possible, and certainly there is no *prima facie* reason why it should not be so.

I conclude this chapter then with the claim that we have seen how it is that we might possess scientific theories which do not use any platonistic mathematics at all, reinforcing the view that the function of this mathematics is a purely extrinsic, representational, one which facilitates the deriving of empirical consequences and predictions from empirical premises whilst remaining contingent to those derivations. In essence the discussion of the motivation for, and content of, the mapping account has been completed in chapters two to four, and the view that the role of mathematics is only a representational one has been sufficiently argued for. In chapter six, as I have said, I will discuss some of the metaphysical issues that I have glossed over in both this chapter and the previous one. But before turning to such metaphysical issues I want to consider some objections to the mapping account that have been raised by Mark Steiner, along that lines that it cannot effectively account for what he calls the descriptive applicability of mathematics. I trust that my development of that mapping account and that fact that the role of platonistic mathematics in science need only be a representational one has been persuasive hitherto, and that some more subtle issues can now be explored in chapter five.

Chapter 5

My claim is that an anthropocentric policy was a necessary factor...in discovering today's fundamental physics.
– Mark Steiner (1998)

The Descriptive Problem of Applicability and its Solution

This chapter confronts a philosophical problem of applicability which, it is claimed, goes beyond a representational role for applied mathematics and poses a problem for the mapping account. The problem in question is referred to by Mark Steiner as the ‘descriptive problem of applicability’, and is introduced in section 5.2. Subsection 5.2.1 covers the sort of reasoning that is central to the descriptive problem, namely Pythagorean reasoning, and discusses the most notable examples of it, viz. Pythagorean analogies and Pythagorean faith. Subsection 5.2.2 gives three examples of Pythagorean reasoning that might be thought to yield descriptive problems of applicability. 5.2.3 is a taxonomy of the species of descriptive problem, under three categories. After the problem is outlined, discussion and suggestion of solutions take place in section 5.3. Subsection 5.3.1. rejects the possibility of anthropocentrism as a solution to the descriptive problem. In subsections 5.3.2, 5.3.3. and 5.3.4 possible solutions to the descriptive problems – of the first, second, and third category respectively – are discussed. I conclude that since each of these problems is successfully met there is no challenge to mapping account from the perspective of this problem.

5.1. Chapter Introduction

The descriptive problem is a putative philosophical problem of applicability which Mark Steiner, most recently in his monograph *The Applicability of Mathematics as a Philosophical Problem*, has drawn attention to. This problem, Steiner argues, arises principally from a type of reasoning in science which he calls ‘Pythagorean reasoning’. In this chapter I aim to clarify Steiner’s account of this reasoning, and present it in the most reasonable and charitable way possible. I do agree with Steiner that there is such a thing as Pythagorean reasoning, though disagree with him about the degree to which one aspect of this reasoning, Pythagorean analogy, does not depend on empirical similarities. As such I present a more cogent account of Pythagorean analogy, and I classify the descriptive problem of applicability into three sub-categories, comprising respectively Pythagorean analogy, Pythagorean faith, and the category of ‘off the shelf’ applications, namely the pre-existence in mathematics of many of the mathematical theories that are required for the development of science, especially theoretical physics.

I argue ultimately that each of these problems can be met without invoking any connections between mathematics and reality that go beyond either the mapping account of applicability or the well-trained physical intuitions of scientists. This is in opposition to the anthropocentric response to the descriptive problem that I argue that Steiner is committed to. The purpose of this chapter is to show that the philosophical problems of applicability do not outrun the mapping account, that it is possible to undermine a view that would pose a problem for the adequacy of that account. Before this chapter commences, I just have one terminological observation to make, namely that the philosophical problems discussed in this chapter arise more with surprising discoveries and novel predictions than with description, but since ‘descriptive problem’ and not ‘discoverability problem’ or ‘prediction problem’ has entered the literature now, I feel obliged to make use of this faintly misleading term.

5.2. The Descriptive Problem of Applicability

The descriptive problem of applicability concerns the fact that mathematics, a subject many branches of which have no obvious connection with empirical phenomena, plays a major role in the description and discovery of seemingly true empirical theories, and the prediction of novel empirical phenomena.⁵¹ Specifically the problem is that our mathematical knowledge, knowledge utterly unlike our natural-scientific knowledge, enables us to gain natural-scientific knowledge in a way which *appears* to go beyond any explanation of applicability as a matter of just the utilisation of the representational capacities of mathematical theories in order to greatly simplify the description of empirical attributes their magnitudes and relations, and the making of physical predictions. I argued in chapter two that the role of mathematics in the making of predictions, deriving of laws, etc, is a purely representational one, with which Steiner would seem to agree for the most part, though not in all cases. Thus the descriptive problem does not concern the possibility of using mathematics in making predictions *per se*, but only in certain novel or surprising special cases in which it might appear that the role of the mathematics is more than representational.

I have already argued against the idea that platonistic mathematics can genuinely explain empirical phenomena, but the descriptive problem left unsolved would leave open the possibility that mathematics, whether construed platonistically or otherwise, has *some* connection with the physical world that entails that there is more to applicability than the mapping account would have us believe. Before I say more about this problem, I want to point out that it is not a single problem. Rather 'descriptive problem' is an umbrella term for a variety of issues, including the making of fruitful mathematical analogies between equations describing empirical phenomena; the fact that the equations in empirical theories can have solutions that we would not expect to find in nature but that we look for

⁵¹ This caveat covers such branches as geometry, which originated in a concern to describe the empirical world.

anyway; and the seeming convenience that mathematics is so useful in describing the world and making predictions in certain cases, and is moreover often found ‘ready to use’ by physicists and other scientists. Before I go into the details of each of these categories, I want to say a little about the sort of reasoning that generates the descriptive problem of applicability.

5.2.1 Pythagorean Reasoning as a Source of the Descriptive Problem:

Pythagorean Analogy and Pythagorean Faith

Steiner makes the case that Pythagorean reasoning gives rise to two forms of the descriptive problem, concerning (Pythagorean) analogy and (Pythagorean) faith. He argues that Pythagorean reasoning does not *merely* consist in reasoning from the empirical to the empirical via the mathematical for the purposes of simplifying theories, making many predictions etc – this sort of reasoning is used in all applications of mathematics, and is, as the explanation of the mapping account of applicability in chapter two made clear, and as I mentioned in 5.2, relatively straightforward – but rather feels that there are other examples that are not amenable to such a mapping-account based treatment. The most prevalent type of Pythagorean reasoning is, Steiner tells us, Pythagorean analogy. This is the sort of reasoning that involves moving from one equation describing one phenomenon to another equation describing another phenomenon, based *only* on mathematical similarity, or analogy, between the equations. One account of Pythagorean reasoning Steiner provides in his book involves two functions linked only by differentiation, although he does not give any examples of this.⁵²

⁵² Steiner explains one way that a physically real equation E may be derived, but it does not seem to me that in actual fact any of the examples that Steiner gives involve this analogy-via-differentiation, and it is not at all clear that it has ever been used: “a standard way to “derive” a differential equation is to begin with a function f , already known to be “physically real”; and then, by differentiating f , find an equation for which f is a solution. The assumption that another solution, g , of the equation, is also “physically real” is...an *analogy* between f and g , mediated by the equation. The analogy becomes Pythagorean if f and g are physically *disanalogous*, so that *only* the equation links them.” (Steiner 1998 p.76, emphasis in original). The problem with this is that since the equations describe physical phenomena, the phenomena which analogous equations describe are likely to be similar in relevant

I do however think that there is a serious problem here. This is the idea that scientists make analogies where the physical phenomena are completely physically disanalogous. For this is simply absurd, and the examples of Pythagorean reasoning which Steiner gives do not support it. We will see some of these in more detail below, but let us jump ahead briefly to the Schrödinger case, Schrödinger made an analogy from a monochromatic light-wave equation to obtain another equation, the Schrödinger wave-equation for the electron. But he did not make an analogy between two phenomena which were linked only by the equation, they were linked by empirical similarities, i.e. both were wave phenomena. And moreover, does it even make sense to say of any two phenomena that they are linked only by an analogy between the equations describing them? If an analogy is possible between the equations this must be, at least in part, because the phenomena are similar in some respect.⁵³ A fruitful analogy suggests a similarity. If the phenomena weren't similar there would be no reason to think it would be possible to make an analogy in the first place. I am not the only person to take issue with this. Michael Liston echoes my view somewhat in his extended critical study of Steiner's book, where he states "I believe there are reasons to be cautious about [Steiner's] interpretation of the evidence" (Liston 2000, p.200-201). For instance, regarding the scientists whose breakthroughs Steiner attributes to Pythagorean reasoning in the no-physical-basis sense, Liston has much to say:

respects, and so the phenomena will not, in fact, be physically disanalogous. Hence my alternative presentation of Pythagorean analogy.

⁵³ It has been pointed out to me although in the natural sciences an analogy may be grounded in empirical similarity, this may not be the case in other disciplines, such as economics, where the similarities between phenomena may be only manifested in structural similarities between the abstract structures that are the subject matter of the theories of these phenomena. One response to this could be to widen our notion of 'empirical similarity' to include such structural similarities, that is, to accept Steiner's characterisation of Pythagorean reasoning, *contra* what I have argued above...If we baulk at this we will be committed to finding a concrete basis for the similarity in each alleged case. If, as seems likely, part of the problem is that the empirical situation is extremely idealised, so that the abstract structure involved is the structure used by the theory of the idealisation (since there may be no adequate direct theory of the empirical phenomenon itself for whatever reason) then this similarity may be explicable in terms of whatever empirical considerations it is that justify using the idealisation to represent the empirical situation in the first place.

In his groundbreaking quantization papers, *Schrödinger* reasoned by exploiting the ideas of de Broglie that he traced back to Hamilton and Klein. There is a physical basis in wave optics (pointed out by Klein) for Hamilton's analytical generalisations of ray optics (Liston 2000, p.201)

[*Schrödinger's*] reasoning was guided by the optical-mechanical analogy, an analogy based on [Pythagorean] considerations believed by him to be grounded in physical considerations (Liston 2000, p.202).

Heisenberg, who of all these theorists was probably the most open to innovation, to exploring different paths, and to breaking with classical ideas, was initially guided by classical physical models (Liston 2000, p.202)

Liston says more, but I think that is enough to motivate a reformulation of Pythagorean analogy. Although Steiner's characterisation of such analogy is either incoherent or irrelevant, Pythagorean analogy is still a useful concept. For 'Pythagorean' can serve to pick out a subset of applications of mathematics that contain some novel or surprising elements, or perhaps the transformation of one equation into another. As such, I am going to suggest that we ignore Steiner's clause that for an analogy to be Pythagorean the phenomena described must be disanalogous, and give the following definition, even if Steiner would not accept it:

(PA) An analogy is Pythagorean *iff* the scientist begins with one equation which describes a physical phenomenon (to a greater or lesser extent) and operates on this equation to produce another equation similar to the first in some relevant respects, for the purpose of describing a phenomenon that is similar in relevant respects to the phenomenon described by the first equation.

The analogy made is that they both describe physical phenomena that are similar in some sense, and so their equations should be similar in some sense. Moreover if we obtain one equation from the other by operating on the first, some degree of

similarity between them should not be surprising. It is clear that both the Maxwell and Schrödinger examples considered below are cases of this sort. Maxwell began with Ampere's and Gauss's equations for electricity and magnetism and modified them to get one unified equation to describe electromagnetism. Schrödinger began with an equation for monochromatic light waves and modified that equation to get a wave equation for the electron. In both cases the similarity with the equations that were the starting points is obvious, as is the similarity between the phenomena being described. I shall argue below that this similarity implies that there is nothing perplexing going on here and indeed is what makes the analogy possible, and that in large part the *reason* that there is nothing mysterious is because of the work done by the mapping account of applicability, which together with the plausible proposition that 'similar empirical situations should be described similarly', leaves nothing to be explained.

There do however appear to be degrees of Pythagorean analogy, though I submit that these degrees do not denote any difference in kind. Steiner argues that a more extreme example of Pythagorean analogy is *formalist analogy*, a method used in quantum mechanics and not to be confused with anything in the eponymous branch of the foundations of mathematics. The focus of formalist analogy is the formalisms of mathematics and physics themselves. When making formalist analogies, physicists take previous formalisms and extend them, subject to certain formal restrictions, hoping that the formal restrictions will cash out physically and that the extension of a previously successful formalism will yield an equally successful formalism in a new case. Steiner asserts that "perhaps the most blatant use of formalist reasoning in physics was the successful attempt by physicists to 'guess' the laws of quantum systems using a strategy known as quantization" (Steiner 1998 p.136). Quantization is the attempt to deduce quantum mechanical laws from classical laws, and has provided the basis for much work in quantum mechanics. It works by treating quantum systems classically, and converting the classical description of the system into a quantum description, essentially 'wave-ising' the classical particle models. This is done by replacing certain variables in the equation with quantum operators.

What is interesting (and formalist) about this is that the quantized equation has the same *form* as the classical equation. This quantum-mechanically-motivated conversion of a classical equation into a quantum mechanical equation is been enormously successful in quantum mechanics, and incredibly useful in physics, despite the fact that it may be thought that there was *prima facie* no reason to suspect that the classical equation had a direct quantum mechanical counterpart with a very similar form, especially given the false initial assumption that the quantum system obeyed classical laws. However, I do not agree with this *prima facie* reasoning, since there is large degree of similarity in many respects between classical and quantum systems, and thus we would expect a certain degree of similarity between their equations. This is not just a bold assertion made by an inexperienced philosopher, it is in fact an accepted physical principle, the correspondence principle, which was vigorously advocated by Niels Bohr: “when considering phenomena in the regime of classical physics, the results of quantum mechanics must give rise to the appropriate classical physics results”.⁵⁴ In other words if, say, General Relativity says a result should be x , then quantum mechanics should say this too: the quantum equations should correctly describe classical phenomena in classical (e.g. macroscopic) situations. It should occasion no surprise that this is in fact the case when the quantum equations are obtained from the classical equations through substitution of some variables for others. There is no deep mystery here despite Steiner’s protestations otherwise (ibid. p.97-98).

Another sort of Pythagorean reasoning, and perhaps the most interesting, concerns those instances where the mathematics tells us something we did not expect, or even feel should not be possible, given what our other theories tell us – in other words the ‘spooky’ cases (cf. Weinberg 1986). This sort of Pythagorean reasoning I shall call ‘Pythagorean faith’. Steiner gives a good characterisation of Pythagorean faith, though he does not distinguish it as such and it is lumped together with his discussion of Pythagorean reasoning:

⁵⁴ <http://www.marts100.com/correspondence.htm>

(PF) A belief is an example of Pythagorean faith *iff* an "...equation E has been derived under assumptions A [and] the equation [E] has solutions for which A [is] no longer valid; but *just because they are solutions of E*, one looks for them in nature". (Steiner 1998 p.76).

The degree of faith involved depends on the extent to which the solutions contravene the background assumptions, and how strong these background assumptions are. Pythagorean faith played a role in, *inter alia*, the Maxwell example considered below. There the background assumptions A in that example are that (1) it should be possible to make the known laws of electricity and magnetism consistent with the law of charge conservation, and (2) that a magnetic field should only arise when there is an actual 'real' current flowing. Maxwell's equation implied that there could be a magnetic field even in the absence of an actual flowing current, which contravened (2), an assumption under which the equation itself had been derived. Nevertheless despite this some scientists did successfully look for examples of a magnetic field in the absence of a real current, and this was due in large part to the unifying power of the equation and the scientists' seeming faith in the equation despite their previous training.

Steiner states that a key element in Pythagorean reasoning is "looking for solutions [to equations] in nature even where there is reason to doubt their very possibility." (Steiner 1998, p.82). In fact this applies only to some cases, although certainly they are interesting. But for the most part the scientist performs a Pythagorean analogy, that is, modifies one equation or set of equations to explain a physical phenomenon similar in some way to that described by the original equation(s), and is *not* looking for any radically new predictions. If in fact many theoretical physicists did spend lots of time looking for surprising solutions to their equations this would possibly disrupt the progress of normal science. There may seem to be a connection between Pythagorean faith and scientific revolution since many of the cases that Steiner cites as examples of Pythagorean reasoning *qua* Pythagorean faith, including those following after a Pythagorean analogy, are in fact cases that led to scientific revolution, such as Maxwell's prediction of

electromagnetic radiation. This is not the case for all examples of Pythagorean faith however, as sometimes an unlikely solution is sought in order to reinforce normal science, when a theory has been accepted as part of normal science but is still used instrumentally to a degree. Clearly it is desirable to minimise instrumentalism if scientists don't just want to 'save the appearances' but to genuinely explain phenomena. One way to reduce the instrumentalism of an equation for instance is to find that all of the solutions to that equation can obtain in nature. One such example is the Schwarzschild solution, which is useful to scientists because it enables both Newton's laws and deviations from them to be obtained from the Einstein Field Equations. Unfortunately the Schwarzschild solution implies the possibility of the existence of an entity of infinite density. Specifically, if fully physically interpreted, the solution states that when a body shrinks to less than its 'Schwarzschild radius' (a magnitude dependent on its mass) it will collapse into a space-time singularity. Such singularities are clearly extremely exotic objects, but despite their perverse character the faith of scientists in the otherwise usefulness of the solution encouraged them to attempt to discover such singularities. It is theorised that singularities lie at the centre of black holes and several candidates black holes have been isolated, including one such hole at the centre of our own galaxy. Although the motivation to locate such a solution may have been pragmatic, faith must have played some role or else the entire endeavour could have been written off as hopeless – though I by no means believe this faith is in any way mysterious. I shall now discuss three concrete examples of Pythagorean analogy and Pythagorean faith, subsequently proceeding to a taxonomy of the descriptive problem of applicability.

5.2.2. Three Examples of Pythagorean Reasoning

Maxwell's Equations and Electromagnetism. This example shows both a Pythagorean analogy resulting in a new equation, and the result of a Pythagorean-like faith in the equation that yielded an example of a new sort of physical phenomenon previously thought unlikely. Maxwell's development of his equation

was motivated by the observation that the classical laws of electricity and magnetism contravened the law of the conservation of electric charge.⁵⁵ In order to arrive at the more satisfactory situation whereby electricity, magnetism and electric charge conservation could all be accounted for under the same laws, Maxwell made a Pythagorean analogy. He took Ampere's law, and added a purely formal element, the 'displacement current', which at the time was not known to have any physical correlate, but rather stood for the rate of change of an electric field. Maxwell thereby arrived a new law $\nabla \times \mathbf{B} = \frac{4\pi}{c} \mathbf{J} + \frac{1}{c} \frac{\partial}{\partial t} \mathbf{E}$, where \mathbf{B} = the magnetic field vector, \mathbf{J} = the current density, \mathbf{E} = the electric field vector, and c = the speed of light in a vacuum. Essentially, this law says "the curl of a magnetic field is proportional to the sum of the conduction current and the displacement current" (Colyvan, 2001a p.268). 'Curl' is a form of differentiation through vector fields. The second term in the sum is the displacement current – this is the purely formal part of the theory, or at least the part of the theory for which there was no physical evidence at the time. The symbols representing the displacement current were a purely platonistic device, despite that device's physical-sounding name, and even allowed for the presence of a displacement current in situations of zero *real* current (i.e. in the absence of the conduction current), something that the science of the time did not accommodate. The unifying power of the law was such that physicists did attempt to find a physical correlate to displacement current. This was discovered subsequently by Hertz, being namely electromagnetic radiation, which was described perfectly by Maxwell's new law. This new law made it possible to talk about electromagnetism, rather than electricity and magnetism separately, and also resolved the whole issue of the consistency of charge conservation with the other laws.

It might well be asked, as a side issue, how this discovery was different from say, the discovery of Neptune. I think the distinction is clear – in the case of Neptune, observations of irregularities in Uranus' orbit together with the Newtonian gravitational theory implied that there *had* to be a large body affecting the orbit of Uranus. Plugging values into equations already known to be physically real, the

⁵⁵ These 'classical laws' were the laws of Ampere, Faraday and Coulomb.

applicability of which the mapping account can explain (cf. the speculative discussion of Newton's law in chapter two) yielded a precise prediction for where Neptune would be. But in the case of electromagnetic radiation, the phenomenon was not expected to be physically real. Rather the equation's commitment to the possibility of displacement current without real current was tolerated as an artefact of the equation because of its ability to unify electricity and magnetism. The upshot is that in the prediction of electromagnetic radiation some Pythagorean faith was required to even search for a physical solution to the equation, whereas in the Neptune case no faith was required, beyond the faith in the pre-existing scientific theories and methods of the time, and in fact in that case it would have been much more surprising if Neptune *wasn't* there. I shall have more to say on the matter of Pythagorean faith below, where I explain how it can be explained in a way perfectly consistent with the mapping account of applicability.

Schrödinger's Equation and Wave Mechanics. Schrödinger's success in wave mechanics provides another, more recent, example of a Pythagorean analogy. Some years before Schrödinger's work, the wave-particle duality of the photon had been discovered, as exhibited in the now famous two-slit experiment. The physicist De Broglie conjectured that this wave-particle duality is a property of all particles, not just light particles. If true, this would mean all particles could be described as waves, which prompted Schrödinger to attempt to discover the wave equation for the *electron*. As a starting point, Schrödinger took a quantum-mechanical wave equation describing the behaviour of monochromatic light waves (waves which are all of the same frequency), and specified a similar equation with the same form describing a wave of frequency E which corresponds to a particle of energy E . This was the base for the Pythagorean analogy. Once the analogy was started Schrödinger continued to refine the equation at each step, to obtain a satisfactory equation.

The initial wave equation formulated by Schrödinger, based on the equation for monochromatic light waves, was:

$$E\Psi = \left[-\frac{h^2}{2m} \nabla^2 + V(x, y, z) \right] \Psi, \text{ where } \nabla^2 \Psi = \frac{\partial^2 \Psi}{\partial x^2} + \frac{\partial^2 \Psi}{\partial y^2} + \frac{\partial^2 \Psi}{\partial z^2}$$

This is an equation for a sine wave of a frequency fixed by E . But unfortunately this equation was too simple – Schrödinger had to account for the majority of cases where radiation affects the energy level of a given electron and so the energy level is not fixed, where, to speak very loosely, the electron-wave is not monochromatic. To remedy this Schrödinger decided to modify the equation still further and remove energy completely from its left hand side, by substituting for it another expression already known to be equivalent to that left hand side but not containing any energy terms. This worked as follows: Schrödinger took the classical equation for the energy of a particle, where total energy E is the sum of kinetic energy $\frac{p^2}{2m}$ and the potential energy V , and then he differentiated and quantised this classical equation in order to obtain $\frac{\partial \Psi}{\partial t} = -i \frac{E}{h} \Psi$, that is, $ih \frac{\partial \Psi}{\partial t} = E\Psi$. Substitution of the left hand side of this equation for E into the initial equation immediately yielded the (time-dependent) Schrödinger equation, $ih \frac{\partial \Psi}{\partial t} = \left[-\frac{h^2}{2m} \nabla^2 + V(x, y, z) \right] \Psi$, with no energy terms on the left hand side.

The Pythagorean analogy is thus complete, and as far we can see, this proceeded as expected and no surprises were encountered. Indeed we would not expect any, given that I maintain that Pythagorean analogy is straightforward and well understood scientific method that is completely compatible with the mapping account of applicability. That the platonistic mathematics is not doing any more than representational work here is apparent once we abstract away from the complicated physical details of the situation and look at what has actually happened: an equation describing one sort of wave was modified so that it described another sort of wave, by making substitutions in the original equation. Yes the phenomena are different, since electrons and photons are different, and so yes the equations governing these phenomena are different and as such they have different solutions,

but it is plain that despite this the analogy works just fine since the phenomena in question are relevantly similar, and does not at any stage impugn the fact that in both cases all the platonistic mathematics is doing is representing magnitudes of empirical attributes and their relations.

Dirac and the Positron. Dirac's discovery of the positron is the most well known example of formalist reasoning, and was very much inspired by the Schrödinger example just given. Schrödinger made substitutions in the Einstein mass-energy equation to obtain the Klein-Gordon Equation (KGE), $\hat{E}^2 - \hat{p}_x^2 - \hat{p}_y^2 - \hat{p}_z^2 = m^2$, in an attempt to derive a relativistic version of his famous equation for the electron. Unfortunately the KGE can only describe spinless particles such as pions, which seems to rule it out as being an equation for the electron since electrons have spin. Dirac's intuition nevertheless told him that this equation was basically what he needed, and that he should adjust it to meet his needs.⁵⁶ One of Dirac's adjustments was to 'factor' the KGE. The result of this factoring was the Dirac equation:

$$(\hat{E} + \alpha_1 \hat{p}_x + \alpha_2 \hat{p}_y + \alpha_3 \hat{p}_z + \alpha_4 m)(\hat{E} - \alpha_1 \hat{p}_x - \alpha_2 \hat{p}_y - \alpha_3 \hat{p}_z - \alpha_4 m) = 0$$

However Dirac had a problem, because in order for the KGE to be factorisable the *coefficients* would have to anti-commute and their square would have to be one. Only 1 or -1 satisfy the property of squaring to 1. But these numbers cannot be anti-commutative, since this would have the consequence that $-1 = 1$ which is false. Since no other two different numbers have a square of 1 no number can be a coefficient of the factored KGE and so KGE cannot be factorised. Or so it would seem.

Dirac's brilliant idea was not to use numbers as coefficients, but rather another sort of abstract mathematical object, matrices, the multiplication of which is not necessarily commutative. Because in matrix multiplication only certain matrices can be multiplied (viz. where the number of columns of the first matrix has to equal

⁵⁶ See the discussion of intuition in section 5.3.2 below. This intuition would be grounded in Dirac's extensive experience with similar situations and knowledge of what had worked previously.

the number of rows of the second) and because the matrices were 4×4 , the solutions they were the coefficients of would have to be at least 4×1 matrices, that is really *four* solutions. This solution-matrix is known as a 'spinor'. Two of the solutions were known to yield the probability of the spin of the electron. But the remaining solutions were a mystery, especially since they were negative-energy solutions, and would seem to correspond to some sort of anti-electron, the positron, which had never been observed. Dirac nevertheless did believe positrons existed, and they were discovered in 1932 by physicist Carl Anderson. Why is this such a good example of formalist reasoning? Because Dirac took an equation known to be physically real for pions, converted it through *factorisation* into an equation which when equal to zero gave the correct predictions for the electron, and moreover predicted the existence of a totally new type of particle through interpreting some seemingly surplus elements of the solutions to the equation which were only required in order to accommodate the coefficients which were necessary to get the equation off the ground in the first place. I maintain however that the idea that these physicists are just manipulating equations using purely platonistic techniques with little in the way of physical analogue, which Steiner plainly seems to believe, is just wrong, as it ignores the fact that their physical knowledge and intuition is a vital factor in guiding physicists to workable solutions and constructing the equations in the first place, a view I shall substantiate below.

5.2.3 The Taxonomy of the Descriptive Problem

We have seen above that it may be thought that the various degrees of Pythagorean analogy, and the success of Pythagorean faith, give rise to descriptive problems of applicability, that is, problems where it seems platonistic mathematics is delivering more than it should be able to if it really is just a formal system that we use to represent the empirical world and simplify our inferences concerning it. As I have made abundantly clear, I do *not* think that this mathematics is delivering more than is warranted and therefore I do believe the descriptive problem can be solved. And

as I hope the tone of my remarks has indicated, this is not because platonistic mathematics *is* warranted to make gratuitous claims about physical ontology, but because the role of applied mathematics is in fact fully explained by the mapping account of applicability together with scientists' empirical intuition, which is explored in more detail below. I have been promising throughout this chapter to provide a solution to the descriptive problem and I shall imminently.

Before I do so though I want to replace the rambling taxonomy of descriptive problems discussed above with a clearer classification, putting the various forms of the descriptive problem already mentioned, as well as another form of the problem I will shortly introduce, into clearly demarcated categories. A *descriptive problem of the first category* is a putative philosophical problem arising from Pythagorean analogies of varying degree. Thus we would say that any successful Pythagorean analogy generates a descriptive problem of the first category. *Descriptive problems of the second category* are generated by vindicated acts of Pythagorean faith, where a scientist finds a solution to his equation that normal science rules is unlikely.

The *descriptive problems of the third category* are generated by a phenomenon that has not yet been explicitly discussed in this thesis, although there was an earlier example of a third category problem, namely Dirac's appropriation of matrix theory for his factorisation of the Klein-Gordon equation. This third category concerns the pre-existence of the platonistic mathematical theories which are vital to the statement of a given physical theory, or to the prediction of a given empirical phenomenon, but which are taken 'off the shelf' as it were, in that they were not created by the scientist for the purposes of science, but rather by the pure mathematician for the purposes of mathematics. This is an interesting phenomenon, because it can seem so unlikely.

Once we understand that we can usefully apply mathematics to the world, we can begin to construct platonistic scientific theories to efficiently describe and help to explain empirical phenomena. But what frequently happens is either that the physicist develops a mathematical theory and then finds out that the same theory has been previously developed by a mathematician perhaps hundreds of years before, or else the physicist requires a piece of mathematics and before he has a

chance to develop it himself, assuming he has the ability, he finds it ready and waiting for him. The reason this seems so peculiar is that we are not just talking about simple and intuitively applicable theories, that could have occurred to anyone, such as the use of numbers for counting physical objects, but rather highly developed and complex pieces of abstract reasoning whose development independently of the physical problems that give rise to them could appear to some minds to be something of a miracle.

As well as Dirac's use of matrices, the imaginary numbers also furnish an example of a third category problem. Orlando Merino has provided a helpful account of the history of the development of these numbers: an account of them was published by Gerolamo Cardano in 1545 in his *Ars Magna*, after knowledge of the imaginary numbers was imparted to Cardano by the mathematician Tartaglia. Tartaglia had developed them (as components of complex numbers) in order to win a mathematical contest which involved solving certain cubic equations.⁵⁷ Thus a concept introduced to enable a man to win a contest, to solve cubic equations, found an essential use in modern science. It took over two hundred years after their introduction before imaginary numbers were accepted as legitimate mathematical objects by the majority of mathematicians. And here is the Wignerian 'miracle': a concept introduced for pure convenience has found a not just widespread, but an essential and ineliminable use in physics, with phenomena corresponding to imaginary-component-containing solutions of equations (though not, of course, corresponding directly to the imaginary components).⁵⁸ Thus in this case as well I shall try to show that there is no genuine miracle at work here. I can therefore now turn to solving the descriptive problem of applicability.

⁵⁷ <http://www.math.uri.edu/~merino/spring06/mth562/ShortHistoryComplexNumbers2006.pdf>

⁵⁸ See Wigner, 'The Unreasonable Effectiveness of Mathematics'.

5.3 Solutions to the Descriptive Problem

Up till now I have contented myself with giving a fair account of the descriptive problem as it appears in the literature. Therefore it is now time to discuss how the problem can be solved, and I argue that this is easily done once the various categories of the problem are understood and Steiner's credulous rhetoric of 'miracles', 'wonders' and 'mysteries' is done away with. I argue that each category can be explained satisfactorily and in a wholly non-mysterious fashion commensurate with mapping account of applicability. But before I give my solutions I want to discuss Steiner's response to the problem.

5.3.1 Anthropocentrism as the Solution to the Descriptive Problem

One interesting question concerns the implications of the descriptive problem for philosophy in general. This question is 'does the descriptive problem imply that the universe is anthropocentric?'. In other words, does the success of Pythagorean reasoning imply that mankind occupies a privileged space in the scheme of things? It will clarify matters to say a few words about the nature and history of anthropocentrism itself. Anthropocentrism was a common belief in the era before Galileo and Copernicus, an era dominated by Aristotelian physics and Ptolemaic astronomy, when it was widely believed that the earth was the centre of the universe and indeed that it was part of the earth's purpose to be so. As is so well known that it scarcely needs repeating, in the post-Galilean era, the sciences were characterised by the opposite trend, by a significant move away from the anthropocentrism of Aristotle and Ptolemy.

Thus anthropocentrism is by no means an unfamiliar concept, and indeed Steiner argues that in the last one hundred or so years science has once again become anthropocentric, though this anthropocentrism may be subtle. He identifies several forms of anthropocentrism, explicit and implicit, covert and overt. The anthropocentric spectrum includes creationism, which is both overt and explicit, but

Pythagorean reasoning by contrast is claimed by Steiner to be an example of *covert* anthropocentrism since the success of Pythagorean reasoning could suggest a privileged position for human mathematics and its applications, even though few physicists would explicitly claim such a position, even to themselves. Indeed, self-consciously working in the Galilean tradition, it is likely that the majority of physicists would vehemently deny any anthropocentrism. Steiner's goal is to question whether they can consistently do so – if they cannot it could be possible to use some naturalistic argument to establish anthropocentrism. Although Steiner does not make this argument, it would proceed from actual Pythagorean scientific practice to the acceptance of anthropocentrism, and could be motivated if the descriptive problem cannot be straightforwardly solved and if extreme examples of Pythagorean reasoning are as widespread as Steiner maintains. For if we accept that the universe is anthropocentric then the ability of human mathematics to mesh with facts about the universe becomes less enigmatic.

It is important to be clear about the issue of aesthetic considerations in mathematics, as it is quite central here, for Steiner is at root arguing, or wanting to argue, from aesthetic considerations to anthropocentrism. Aesthetic considerations are relevant to which mathematical theorems are proved, and how mathematical theories, including physical theories with a large mathematical component, are stated, and there is no *a priori* reason to hold that aesthetic considerations that are persuasive to humans would be persuasive to non-humans.⁵⁹ It is plausible that no theorem proved by the alien will follow if it does not also follow for the human, at least so long as the human and alien are using the same logic.⁶⁰ Certainly, unless there is some error lurking within our mathematics itself, or within the alien mathematics, the human and alien will never prove inconsistent theorems so long as

⁵⁹ For more on the prevalence of aesthetic motivations in mathematics (and science) see Osborne, H. (1984) 'Mathematical Beauty and Physical Science', *British Journal of Aesthetics*, vol. 24, pp. 291-300, and Sinclair, N. (2011). 'Aesthetic Considerations in Mathematics', *Journal of Humanistic Mathematics*, vol. 1, pp. 2-32.

⁶⁰ At any rate I am assuming this. There is a large literature on the possibility of logically alien thought and related issues, but since the descriptive problem arises when human and alien logic are the same, it will *a fortiori* arise when this logic is different.

they do not make mistakes. Rather, the problem is how the human and alien *practice* mathematics.

The methods of human mathematics are guided by considerations of both aesthetics and convenience. But what is beautiful or convenient to a human may not be so to an alien. Human mathematical methods are restricted by what human mathematicians can comprehend, and many calculation systems arise out of human factors, the fact we have ten fingers on our hands giving rise to our base-10 notational system, for example. Aliens may well have very different systems, and their (for the sake of argument) superior intellect may see convenience and aesthetic value where human mathematicians witness only chaos. Therefore aliens might have not only made huge advances in areas we have only dreamt of, or cannot even dream of, but might have focused on entirely different areas, possibly to their detriment and possibly to ours.

Here we have a potential dilemma that arises from the descriptive problem: either anthropocentrism is false and the fact our aesthetic impulses are partially responsible for the platonistic mathematics that has contributed so much to physical discovery does not suggest a privileged place in the universe, but is rather a coincidence, or it does suggest this and anthropocentrism is true. I do not believe we can afford to impale ourselves on the second horn without getting embroiled in discussions concerning pre-established harmony. Indeed aversion to such a view has been ubiquitous among responses to Steiner's work, where reviewers have raised important questions. For instance, even Mark Colyvan, the reviewer most enthusiastically sympathetic to Steiner's work, writes:

...if Steiner is right and the universe *is* user-friendly (that is, human consciousness holds a privileged place in the universe), what are the consequences of this? Although Steiner falls short of drawing any theistic conclusions, such conclusions are beckoning! (Indeed his thesis appears to be closely related to design arguments). (Colyvan 2000 p.394)

This is a view echoed by Peter Simons in his review:

[Steiner's argument] is prey to a variant of Hume's objection to the argument from design, which is that we cannot infer from a single case, the universe we actually inhabit, that it is unexpectedly "user friendly..." (Simons 2001, p.184)

I cannot see how Steiner might hope to get away from the anthropocentrism he discusses: "[f]or what seem to be anthropocentric methods of discovering laws are so entrenched and widespread and so spectacularly successful that they cannot simply be dismissed" (Steiner 1998 p.73), and indeed he concludes "[t]he world, in other words, *looks* 'user friendly'..." (ibid.p.176), which I read, not uncharitably, as an endorsement of anthropocentrism. If he does not endorse it he certainly makes no attempt at all to explain why this 'seeming' user-friendliness, this anthropocentrism, is not in fact genuine. But as I have said, *I* have no wish to go down the anthropocentric road. This leaves the first horn of the dilemma: it is a coincidence that our aesthetic impulses have in some cases led to mathematics that has played a vital role in various empirical descriptions and predictions, though not a coincidence that they have had an impact on our development of mathematics. But this coincidence is not inexplicable, which is reassuring as philosophers are notoriously intolerant of unexplained coincidences.

The bulk of this chapter focuses on showing how the mapping account can solve, or at least be consistent with the solutions of, the various forms of the descriptive problem, and so the role of aesthetic impulses is largely neglected. It would be a mistake however to say nothing more about the matter, since it provides some motivation for the descriptive problem in the first place. The answer is simple however: symmetry. Human aesthetic impulses tend towards the symmetrical and much symmetry has been discovered in nature, and provides a clue to the form of natural laws.⁶¹ Beyond this our aesthetic impulses have little role to play in the development of mathematics, though they do have an influence of the specific proofs of particular theorems, where beautiful proofs, 'proofs from the book', to use Erdos' phrase, are preferred to 'ugly' ones. Some branches of mathematics such as group theory are very concerned with symmetry, and with which transformations will preserve various symmetries, and group theory

⁶¹ See Feynmann (1965) chapters 4 and 7 for more on symmetry in physical laws.

especially finds a great deal of use in theoretical physics. But for the most part the role of aesthetics here has been exaggerated – the discovery of such things as the integers, the reals, and the complex plane, all of which have a vital role in applied mathematics, owe little to aesthetic considerations.

I have two further points to make. Firstly, it is perfectly possible that in the scheme of things our aesthetic impulses are not particularly useful in motivating the mathematics that is conducive to science, and that aliens who have different aesthetic impulses have obtained a more advanced and more complete physics. No *a priori* considerations can rule this out. Secondly, I have said the main aesthetic impulse here is the drive to symmetry and symmetry is also found to be fundamental in the way the universe is structured. But to jump from this to anthropocentrism is about as valid as jumping from the fact we have evolved senses that can interpret the world to anthropocentrism, i.e. not at all (the ‘intelligent design’ posse notwithstanding). Therefore, I believe, we should be content to grant that aesthetic considerations aren’t especially problematic, and do not support anthropocentrism.

Moreover it is not evident that aesthetic impulses are the only, or main, drive in the development of mathematics: in a recent paper Sorin Bangu (2006) has argued that in many cases our mathematical impulses are quite explicitly anti-anthropocentric. Bangu’s argument is these anti-anthropocentric drives are clear in two very important mathematical developments, the rejection of definabilism and the acceptance of the axiom of choice. The rejection of definabilism, led by Euler, was the rejection of the idea that all functions must be definable in an explicit and uniform way. This entailed that a great many more functions could be admitted as part of the subject matter of mathematics, and lead eventually to the notion of a function as an arbitrary correspondence. The acceptance of the axiom of choice meant that mathematicians accepted the existence of a set with an element from every set in a range of sets (a ‘choice’ set), even in cases where the choice set was not explicitly defined. This axiom has been vital in the proofs of many important mathematical theorems such as the Banach-Tarski Theorem and the Baire category theorem. Without these anti-anthropocentric developments we would lack a great

deal of mathematics, mathematics which is no less likely *a priori* to find empirical application than that mathematics based on aesthetic impulses, even temporarily allowing that such impulses are anthropocentric. However it is now time to get on with addressing how the mapping account can meet the various forms of the descriptive problem.

5.3.2 Solving Descriptive Problems of the First Category.

Recall that a *descriptive problem of the first category* is a putative philosophical problem generated by a successful Pythagorean analogy. The reason such an analogy appears to generate a descriptive problem is that we take one equation describing one phenomenon, manipulate it mathematically, and obtain another equation describing another phenomenon. Thus it looks as though mathematics has some power to give us knowledge of the world by non-trivially transforming our existing descriptions of the world into other descriptions of different parts of the world. Moreover, thinks the believer in descriptive problems of the first category, this power cannot be explained in terms of the representational capacities of mathematics because the mathematical manipulations we perform may lack an empirical correlate, which is to say they do not correspond to any empirical connection between the equations, and so nothing is being represented. Besides being extremely questionable given how it was seen in the previous chapter that many abstract mathematical operations do represent empirical relations, I submit that this view of the descriptive problem is completely false. The descriptive problems of the first category can only be motivated by a loaded statement, which may be persuasive to the unwary but which is certainly not accurate.

See how the problem disappears when what is going on is understood differently, and all the facts are accounted for: we take an equation describing one phenomenon. We manipulate it mathematically and obtain another equation for another phenomenon. But the phenomena in question are related. So we would expect them to be described by similar equations, and we would expect that one

equation can be transformed into another equation through a certain degree of manipulation. Take the Schrödinger example: monochromatic light is a wave. If wave-particle duality obtains, the monochromatic light also exists as photons of a fixed energy level. The conjecture is that wave-particle duality obtains for all quantum particles. So we would expect particles such as electrons to exhibit it, and thus be describable using a wave equation. Thus we would not be surprised if there was a high degree of similarity between an equation describing one wave and another equation describing another wave. And we would also not be surprised if those equations could be transformed into each other, since the equation describes the wave and so substituting different variables etc in the equation for others keeps the structure of the equation, whilst enabling it to describe different, though similar, waves.

Moreover, as we already have a background of equivalent definitions of empirical attributes in terms of other, perhaps more fundamental, attributes, it is evident that expressions can be substituted for other extensionally equivalent expressions *salva veritate*, which may, even to a high degree, *appear* to change the form of the equation somewhat, but which has in not actually altered its form at all, just how it is displayed. For example, to obtain his equation Schrödinger substituted a quantum mechanical version of the equation for the energy of a particle for the classical version of the equation, but the form of the equation remained the same, for it was still the energy of a particle that was being defined by the right hand side of the equation, though this energy was now understood in quantum mechanical, and not classical, terms.

There is no more mystery here than if I give a description of rugby and then supply a description of American football by altering my description of rugby. Both rugby and American football are similar to quite a high degree so we would expect their descriptions to be quite similar and for it to be possible to transform one description into another. The mathematical manipulation here is by no means an indication of a special power of mathematics that is to be explained by appeal to some sort of anthropocentrism: the physicist starts with one description of a phenomenon. And the physicist knows (well enough) what the other phenomenon

he wants to describe is. So with enough physical intuition and formal skill, the cultivation of which is, after all, the point of his extensive research training, the physicist can obtain a description of the second phenomenon from the description of the first. Of course he may also be able to invent a description of the second phenomenon from scratch, but this would be a lot of extra work when he already has access to a description of a similar phenomenon. For that is how much science in its 'normal' phase proceeds – by piecemeal expansion and development of concepts and descriptions that are accurate within certain limits and which can therefore be constructively built upon. The intuition appealed to here may seem a little mysterious, especially given views such as Einstein's that

the supreme task of the physicist is the discovery of the most general elementary laws from which the world-picture can be deduced logically. But there is no logical way to the discovery of these elemental laws. There is only the way of intuition... (Einstein, in the foreword to Planck, 1981).

However Einstein continues “[intuition] is helped by a feeling for the order lying behind the appearance, and this *Einfühlung* [literally, empathy or 'feeling one's way in'] is developed by experience.” (ibid). Developing a full account of this experience would no doubt be a complex and challenging task, but it seems clear that the experience, the ground of the intuition, is not in principle mysterious even if it is difficult to fully explain at present. We will see that this intuition plays a role in second category problems as well.

So to conclude, descriptive problems of the first category present no issues for the mapping account of applicability, together with an appreciation of scientific intuition. If we avoid misleading statements of the way Pythagorean analogy works, then the use of the mathematics in the Pythagorean analogy does not go beyond the representational role which the mapping account allows it. The extent of this role is evident from the discussion in chapter two of this thesis, where it was shown how mathematical structures can represent relations of attributes of empirical entities. facts. Yes, appealing to, and using, platonistic mathematics greatly helps the scientist

and enables him to abstract from phenomena, to appreciate structural similarities between phenomena, and describe phenomena accurately. But it is simply not true, as I showed in the last two chapters, that one's account of the physical world need make any appeal to platonistic facts. For knowing that both photons and electrons have wave-like properties is empirical and not mathematical knowledge, and, at the risk of labouring the point, we would expect the equations describing those phenomena to be similar to the degree that the phenomena are similar.

5.3.3 Solving Descriptive Problems of the Second Category.

Descriptive problems of the second category, you will remember, are generated by vindicated acts of Pythagorean faith, 'where a scientist looks for, and finds, a solution to his equation which normal science ruled was unlikely'. Can descriptive problems of the second category be solved in the same way as descriptive problems of the first? Evidently the answer to this question is in the negative, since the faith of scientists in their equations is a subjective matter to which the mapping account does not contribute, and regarding which Pythagorean analogy does not really play a role. The aim of this section is therefore to explain, or at least to give the outline of an explanation, of how it is that scientists' faith in their equations results in their successful conjecturing of solutions to these equations that represent totally new sorts of empirical phenomena. It is easy to why, at first glance, it might seem that this is a problem a good answer to which can be found if we believe in the anthropocentrism of the universe – that there is some deep connection between our platonistic mathematics and physical reality such that the mathematical form of the equation has empirical implications. But, I submit, this is surely to jump the gun. Equations describe the behaviour of phenomena that scientists are familiar with, and to be acceptable the equation must successfully describe all known instances of the phenomenon. It must also describe unobserved instances of the phenomenon if the scientist is to do more than 'save the appearances'. The extent to which this is justifiable is the subject of the philosophy surrounding the problem of induction, but

the problem of induction is not our problem – although it may seem superficially similar insofar as both the second category descriptive problems and the problem of induction go beyond what is observed.

To return to the issue at hand, the equation might be satisfied by certain solutions that seem to be physically unlikely in the sense that these solutions satisfy the formal constraints of the equation but have properties that if physically instantiated would put them at odds with the views of the scientific community. So why should these solutions be expected to obtain, when all they have in common with the physically observed solutions to the equation is that they are also solutions of the equation? The answer has to be that *there is no indubitable reason to expect this* unless the scientist has some sort of non-observational knowledge independent of what has been currently observed. The anthropocentric answer to this problem suggests that there is, or at least could be, such knowledge, but this is precisely what I deny. In order to motivate this denial it is essential to see why acts of Pythagorean faith might exist at all, as well as why many of them might actually obtain.

The answer is found in the minds of the scientists themselves, but it does not concern a mysterious connection: a scientist who has worked for a long time on a certain equation that proves successful in describing a known phenomenon is likely to be encouraged by this, perhaps to the extent of asserting that it is actually satisfied by physically unlikely solutions. Anyone that has worked hard on a successful theory is familiar with the feeling of exultation and the thought ‘brilliant, I bet this works for more things’. Indeed it is by no means the case that this applies exclusively to the scientist who develops the equation – another scientist, familiarising himself with said equation, may be sufficiently impressed to hypothesise that all solutions to the equation obtain, perhaps because the equation solves a problem the second scientist is working on, unifies a certain amount of theory, or so on. We likewise cannot dismiss the relevance of other human emotions to acts of Pythagorean faith, such as arrogance or overconfidence in his or her abilities or intuitions.

This brings us back to the subject of physical or empirical intuition, which, arrogance, overconfidence or hubris aside, may well be the most universal reason

for acts of Pythagorean faith by scientists. Scientists, theoretical physicists especially, spend a large fraction of their time thinking about, and reflecting on, the structure of the empirical world in formal and abstract terms. It is therefore to be expected that their physical intuition is honed, that they are familiar with patterns they observe in nature, that they appreciate when their equations describe phenomena in ways consistent with these patterns, and that they eventually develop a 'gut' feeling that the solutions to these equations obtain, even the unlikely solutions.⁶² There need be no more to it than that.

For example, Dirac felt that if there were solutions corresponding to *every* component of the 4×1 matrix (called a 'spinor') that was the solution to his equation possessing 4×4 matrix coefficients, then this would exemplify a certain amount of symmetry that physicists commonly observe in the universe, and that therefore such solutions should be possible.⁶³ But he could not have *known* that this would occur because the positron had not yet been observed. And this is precisely where Pythagorean faith entered the picture: the symmetry of the theory may have required the existence of physical correlates to all the components of the matrix, but as the history of science shows us beyond doubt, scientific theories are often imperfect or incomplete, and just because the existence of a certain phenomena would make them more symmetrical or 'round them out' this does not *entail* that they exist, it took an act of faith to conjecture this.

That it takes a certain faith in the equation to conjecture such solutions does not imply that successful acts of Pythagorean faith are statistically insignificant, indeed it is likely that they are statistically *significant*, especially compared to, say, a computer program that produced an output of random scientific theories. But this success can unproblematically be attributed to scientists' physical intuition, and scientists are after all not producing random theories. I don't invoke this intuition as a mysterious faculty, it is simply the product of years of scientific research training,

⁶² In fact, the mapping account outlined in chapter two explains precisely why it is that thinking about mathematical patterns helps physical intuitions, namely that mathematical structures represent physical structures such that thinking about mathematical structures in certain patterns leads naturally to thinking about empirical structures exhibiting these patterns.

⁶³ Steiner (1998) pp. 159-160.

and is the point, we might say, of such research training, and grounded, as Einstein pointed out above, in extensive experience. Therefore, a descriptive problem of the second category can be explained in terms of physical intuition without appeal to either anthropocentrism or indeed any other enigmatic connection between mathematics and reality.

5.3.4 Solving Descriptive Problems of the Third Category.

Descriptive problems of the third category are those involving the knowledge of the requisite platonistic mathematical structures independently of their application to empirical phenomena. It seems clear that these problems cannot be solved by the above methods, but then I submit that it is equally unclear that there is really a 'case to answer' here. Before we move on to how to dispense with third category problems, I want to give two examples.

Matrix Mechanics. Matrix mechanics was a forerunner to the wave mechanics developed by Schrödinger, and indeed the term 'Quantum Mechanics' derives from the formal equivalence of wave and matrix mechanics. It is an instance of a third category problem because of the surprising application of matrices to quantum phenomena. Matrix mechanics was developed by Born, Jordan and Heisenberg in response to the famous problem of the missing spectral lines of hydrogen, although Heisenberg was the principal architect, after he had a famous revelation: 'It was about three o' clock at night when the final result of the calculation lay before me. At first I was deeply shaken. I was so excited that I could not think of sleep. So I left the house and awaited the sunrise on the top of a rock'. The core of Heisenberg's revelation was to focus on observable properties of atoms, and realise that these observables might have non-commutative properties. Given that non-commutative properties are nicely modelled by matrices, the introduction of matrices makes a lot of sense – interestingly matrices had very little application in physics hitherto. The term 'matrix' is an umbrella term – the specific sort of matrix with which

Heisenberg was concerned was the ‘Hermitian’ or ‘self-adjoint’ matrix, “...a square matrix with complex entries which is equal to its own conjugate transpose, that is, the element in the i th row and j th column is equal to the complex conjugate of the element in the j th row and i th column, for all indices i and j ”.⁶⁴ An example of a Hermitian matrix is:

$$\begin{pmatrix} 3 & 2+i \\ 2-i & 1 \end{pmatrix}$$

The first diagonal always consists of real numbers. Call the Hermitian matrix ‘A’. A nonzero vector B is an eigenvector iff there is some number λ such that $AB = \lambda B$. λ is an eigenvalue of A, and a scalar which the vector is multiplied by. B is the eigenvector associated with λ . For the matrices with which Heisenberg is concerned, the eigenvalues are all real numbers. The power of matrix mechanics was such that it was the first remotely complete and accurate description of quantum phenomena, but not only did the matrix formulation of Quantum Mechanics agree with the known quantum data, it also provided many interesting results. For example, the uncertainty principle follows under a suitable interpretation from the non-commutativity of the matrices, and matrix mechanics permits the derivation of physical results concerning the spectra of atoms which are more complex than the simple hydrogen case, e.g. the case where just a single electron ‘orbits’ the nucleus. What is so remarkable is that all this detailed mathematical matrix apparatus for describing these empirical phenomena already existed independently of the empirical phenomena it was used to address, *predating Heisenberg’s work by around sixty years*.

Eigenvalues of Hermitian Matrices and the Riemann Zeta Function. This is another example of mathematics having ‘been there before’, coincidentally also involving physical interpretations of matrices. In this example the existence of the mathematics was discovered by a physicist by accident, and as far as I know has had

⁶⁴ Source: http://en.wikipedia.org/wiki/Hermitian_matrix

no direct role in any scientific advances, but it is no less remarkable for all that. The mathematician Riemann was concerned with the distribution of the prime numbers and in the course of his work discovered a function, the zeta function, which bore a close relationship to the prime numbers when the function returned a value of zero. Here is the function:

$$\xi(s) = \sum_{n=1}^{\infty} \frac{1}{n^s} = \frac{1}{1^s} + \frac{1}{2^s} + \frac{1}{3^s} + \dots \text{for complex } s \text{ such that } \Re(s) > 1$$

The discovery that this equation describes an empirical phenomenon was the result of informal discussion between mathematician Hugh Montgomery and physicist Freeman Dyson. When Montgomery was explaining his problem to Dyson, and showed him the distribution of the gaps between the zeroes of the zeta function, Dyson observed “that the gaps between pairs of eigenvalues of random Hermitian matrices are likewise distributed. Such eigenvalues have been studied at length in connection with energy levels in the nucleus of a heavy atom under bombardment by low-energy neutrons”.⁶⁵ So a piece of totally pure mathematics, designed without anything empirical in mind at all turns out to describe perfectly the energy levels of certain atoms. The mathematics and physics is extremely complex, such that we should certainly not have looked for, or anticipated, such a correspondence.

Solving the Problem. So how can third category problems be ‘solved’? Take the first example, matrix mechanics. Mathematicians study a whole range of different objects and structures. Around two hundred and fifty thousand new theorems are proved each year, and many new and exotic mathematical objects and structures are discovered or constructed. Heisenberg primarily needed mathematical objects that had non-commutative properties in order to describe certain physical properties he was concerned with. Had, counterfactually, matrix theory not been invented, but instead xyz-theory dealing with non-commutative xyz’s been invented instead, it

⁶⁵ Source: newsletter of the Society for Industrial and Applied Mathematics, <http://www.siam.org/pdf/news/205.pdf>

seems likely Heisenberg would have helped himself to this mathematics, or perhaps have invented matrices himself. (Of course neither of these situations may have obtained and the progress of Quantum Mechanics would doubtless have been severely set back). This could seem more problematic in the Dirac case above, which is really an example of a first, second, and third category problem because not only were matrices required for the description of phenomena, but it was the vector coefficient of the matrix (itself a matrix) which was instrumental in the *discovery* of the positron. However, it seems to me that this was accounted for quite well insofar as the solutions to problems of the first and second categories are concerned, and we ought not to be surprised that the matrix theory in question existed independently of Dirac.

After all, as we have said, there are rather a lot of platonistic objects, and matrices do not even make it into the ranks of the exotic or bizarre. Pedestrian and ubiquitous as they are, it is not a surprise that they were discovered long before their applicability to empirical phenomena became apparent. This same answer cannot be given in the Riemann case, because in this instance we are dealing with a very abstruse entity, viz. the zeta function. This case is a much stronger instance of a third category problem, but recondite though it is, it does still seem solvable. There are a large amount of enormously complex structures in the physical world, and one would expect a physicist to be especially sensitive to those of them they are familiar with. Each empirical structure does of course have a platonistic analogue, since platonistic mathematics is about all self-consistent abstract structures. A physicist or mathematician, when presented with two structures which he has knowledge of, can say whether or not they are the same. And this is what happened in the Dyson case: Dyson knew the structure of the eigenvalues in question, and when presented with the eigenvalues *qua* values of the zeta function he was able to see the structures were the same, that the values of the function and the eigenvalues he was already familiar with were the same. There is nothing in the least bit surprising about this, although it is a coincidence – if Montgomery has been talking to an astrophysicist the similarity would probably have gone unnoticed. It must be the case there are a great many structures exemplified in the physical world which have

not been described by mathematicians (or physicists), the knowledge of which would be very useful for physics. Likewise, no doubt there are many already discovered mathematical structures that would be useful to physicists if they knew of them. Either way, I cannot agree that there is any philosophical problem arising out of this, and I hope the analysis of these examples has convinced the reader of the same. Certainly we would wish to know the extent to which this happens, but it is certainly frequent enough for physicist Steve Weinberg to write “it is positively spooky how the physicist finds out that the mathematician has been there before him or her” (quoted in Steiner 1998, p.13). However something being spooky stops very short of it being a miracle which can justify anthropocentrism, especially as I have offered an explanation in terms of thousands of mathematicians proving theorems about a diverse range of abstract mathematical structures.

There is also another perspective concerning the surprise we might feel that abstract mathematical structures fit the empirical world, namely that abstract mathematical structures and theorems about them have empirical interpretations precisely because a great many of them were constructed for exactly that purpose, and it is not always just a happy accident arising from the plenitude of abstract structures being studied. In very simple cases this is obvious, no one is amazed that geometry is applicable because we all know geometry was originally developed for the purposes of ‘earth measuring’, hence its name. But it is important to remember that this holds true in many non-geometrical cases as well. Newton and Leibniz’ development of the differential calculus for the purposes of describing changes of forces with respect to each other is a non-trivial example here. (Of course in this instance it would be less likely that there would be a third category descriptive problem, as we would be less inclined to regard mathematical structures constructed for empirical purposes as being ‘off the shelf’. But either way, it seems clear that descriptive problems of the third category are nothing to worry about.)

I submit then that all three categories of the descriptive problem of applicability have been solved, and that *a fortiori* the descriptive problem of applicability is not an insoluble problem for the mapping account of applicability

which I have endorsed in this thesis. I turn now, in chapter six, to ontological and methodological issues arising from the mapping account

Chapter 6

In short ... a reasonable interpretation of the application of mathematics to the physical world requires a realistic interpretation of mathematics.

– Hilary Putnam (1979)

Implications for Philosophy of Mathematics: Indispensability, Nominalism, and Idealisation.

In this chapter I explore some of the implications of the mapping account developed in the preceding chapters for wider issues in the philosophy of mathematics including whether the mapping account is committed to abstract mathematical objects; the bearing of the account on the indispensability argument; whether the account can be nominalised; and if idealisations pose any problems for the mapping account. I argue that the account is *prima facie* committed to abstract mathematical objects, and examine the relation of this fact to the indispensability argument for mathematical platonism. However I maintain that there are construals of the mapping account which are both acceptable to and useful for a nominalist. I look at both fictionalism and nominalist views that try to provide surrogates for the mathematical objects that our scientific theories quantify over. Turning then to the issue of idealisations, I argue that idealisations do not pose any problem for the mapping account.

6.1. Chapter Introduction

In chapters two to four the mapping account of applicability has been developed, the dispensability of genuine platonistic explanations of physical phenomena has been argued for, and the details of how nominalistically acceptable explanations of physical phenomena might be possible have been expanded on. In the previous chapter, five, I made the case that there is nothing mysterious about the applicability of mathematics to the physical world. Thus the preceding chapters amount to an argument for a representational conception of applied mathematics in general and for the mapping account of applicability specifically. I believe the treatment has been sufficiently thorough, although this is not to say that there are no philosophical loose ends to tie up. For instance, if a working account of applicability is as important to the philosophy of mathematics as I have suggested throughout this thesis, then we might expect it to have some implications for such traditional concerns in the philosophy of mathematics as the platonism/nominalism debate. I have not much hitherto addressed the ontological issues to a great degree because of a desire to remain neutral with respect to ontology as far as possible when developing the account, in order that the explanatory power of the account would not rest on any particular assumptions about the nature of mathematical objects.

In this chapter I consider whether or not the mapping account is committed to platonism, and examine the relation of the indispensability argument, the most compelling argument for platonism, to that account (section 6.2). I then discuss some nominalist responses to the account in section 6.3. In 6.3.2 I consider the status of the mapping account as an explanation of applicability if one believes that mathematical objects are fictional. In section 6.3.3 I present an alternative nominalist philosophy that allows the mapping account to stand more or less as it was presented in chapter two, but which provides nominalistically acceptable surrogates for the platonistic objects that are referred to by it. Section 6.3.4 discusses the modal structuralist approach to the issue of nominalistically acceptable surrogates, which may be an alternative to that of 6.3.3. But ontological

issues are by no means the only questions raised by the mapping account – further questions arise from considerations about idealisations and are addressed in section 6.4. For example, are there non-physical idealisations in physical explanations, and if so what problems do they or would they cause for the mapping account? Can or even should the mapping account handle the physical idealisations that appear so ubiquitous in science? Section 6.5 forms the conclusion of this thesis.

6.2. Platonist Ontology and Indispensability

Over the last five chapters we have seen the mapping account developed and defended. But so far I have abstained, as I have frequently indicated, and the reader has doubtless noted, from drawing many ontological inferences from or about the mapping account. I can put this off no longer: does the mapping account commit us to abstract mathematical objects? That is, is it committed to a traditional platonist view of mathematics? The most natural reading of the mapping account is a platonist reading because the mappings it uses are mappings from empirical structures to what are usually taken to be *abstract* mathematical structures (frequently the real number structure) which provide the values of measurement of empirical magnitudes.

This attitude of many mathematicians and philosophers of mathematics has contributed to the acceptance of the platonistic view as the standard view of mathematical structures, and thus the platonistic reading of the account is the *prima facie* reading. There is more evidence to support this: representation theorems are heavily involved in the mapping account insofar as they show how mappings are possible. These representation (and uniqueness) theorems are part of measurement theory, a branch of classical mathematics, and involve references to all sorts of sets, functions and numbers which are standardly treated as platonistic objects. Such references are also found in some of the axioms of the various axiom systems that are selected for proving the representation theorems and characterising any

particular empirical attribute that we want to measure or describe mathematically, such as the Archimedean axiom. I explained in chapter two that the mapping account holds that mathematics can be applied in virtue of mappings from empirical structures to mathematical structures, mappings that we know obtain between empirical structures satisfying suitable axioms and mathematical structures *because of* certain representation theorems. This suggests very strongly that the mapping account of applicability is in fact committed to some form of platonism, though it does not preclude there being a nominalist version of the account. If there is such a nominalistic interpretation of the mapping account it follows that applicability may be explainable in terms of something other than mappings into *abstract* mathematical structures. Before getting into nominalistic issues, I want to consider the degree to which a platonist could use the mapping account as an argument for mathematical platonism by arguing for platonism from the putative indispensability of mathematics for the mapping account.

Indispensability to our best scientific theories is a powerful reason to believe in the existence of something. This view has received a great deal of attention at the hands of platonists who argue that references to abstract mathematical objects are indispensable to our best scientific theories and that we should therefore believe in the existence of what they seem to refer to. One common formulation of the argument is as follows, due to Quine (Colyvan 2001, p.11):

1. We ought to have ontological commitment to all and only the entities indispensable to our best scientific theories
2. Platonistic entities are indispensable to our best scientific theories
- Therefore
3. We ought to have ontological commitment to platonistic entities.

The biconditional in the first premise may be weakened to a conditional if we think there may be other reasons for ontological commitment. The use of the 'ought' here is misleading, it suggests that we should, but don't have to, be so ontologically committed, when in fact it must be read much more strongly, to mean that we

cannot consistently accept the scientific theories and not accept the platonistic entities indispensable to such theories. Insofar as Field has managed to at least indicate how platonistic mathematics may be eliminated from the statement of (some of) our best scientific theories, examples of which I discussed in chapter four, I take it that premise two has been seriously challenged and that the indispensability argument has lost much of its force. But two issues remain. Firstly many platonists remain unconvinced by Field's arguments undermining the second premise, and for these philosophers considerations about the first premise of the argument are likely to be more effective in bringing the argument into disrepute. However I am more concerned with a second issue – namely the ontological implications of the mapping account itself. To defuse these ontological implications, I argue in 6.3 that nominalistic versions of the mapping account are possible.

Before looking at this topic, I would like to address the first issue: should we believe the first premise of the indispensability argument? That is, should we believe in all and only those objects reference to which is indispensable to our best current scientific theories? If we believe that science does tell us with at least approximate accuracy what *physical* entities there are, then we are going to be inclined to believe in the existence of these physical objects. And if we are physicalists who believe that all objects and forces are physical objects and forces then this view will have a certain persuasiveness. This much is uncontroversial, or at least if it is controversial, the burden of proof is on the opponent of science *qua* guide to ontology to tell us why. What is more questionable, though, is whether we should believe in the existence of the *platonistic* objects that our scientific theories, stated in their usual form, quantify over. If such objects are genuinely indispensable then there is relatively little room for manoeuvre here by the nominalist who is at all persuaded by naturalistic arguments. But there are different degrees of naturalism. In its strongest form (e.g. that of Maddy 1997) the role of philosophy in science and mathematics is reduced to little more than a sociology of science mathematics, where we accept what scientists seem to be saying. For instance Maddy says “[i]f and when the naturalistic philosopher does turn to metaphysical questions, she is constrained by the conclusions of her naturalistic methodological

enquiries" (p.233) which "takes the actual methods of natural science as its own" (p.183). This version of naturalism takes platonistic commitments are more or less face value. At the opposite end of the spectrum is the sort of view anti-naturalistic Field sometimes seems to be espousing, when he says things like

I believe that synthetic approaches to physical theory are advantageous, not merely because they are nominalistic but because they are in some ways more illuminating than metric approaches; they explain what is going on without appeal to extraneous, causally irrelevant entities". (Field 1980, p.43)

This is anti-naturalistic because metric approaches that involve causally irrelevant entities are precisely approaches contemporary scientists adopt. But there are moderate views between these two extremes that are not anti-naturalistic. For instance, we may also accept, as I do, that scientists do not necessarily think about the metaphysical commitments of their theories and that philosophers are aware of things that scientists are not, such as that if a term appears to refer to an abstract object it does follow that an abstract object is what it refers to. This suggests that what appears to be indispensable may not, on further reflection, be so. Such reflection would not be the business of science, which is not to say that science is not philosophically relevant, or that philosophical views cannot arise out of scientific views.

In keeping with my more moderate view the earlier chapters of this thesis have challenged the idea that references to platonistic objects are genuinely indispensable to our physical theories. What has not yet been addressed though, and what I want to examine now, is whether references to such objects are indispensable to our best theory of applicability, the mapping account. For if they are so indispensable there is nothing to prevent a platonist from constructing an indispensability argument for platonism based on the appearance of such references in the mapping account. I believe such an attempt would be quite misguided however, and lack any force, since part of the support for indispensability arguments is that the practices they are built upon are extremely central to our entire understanding of the way the world works, indeed they form our only real

way of gaining knowledge of the world. No one can claim such status for the theory of applicability, however desirable it might be to explain the application of mathematics to empirical phenomena.

6.3 Can we Nominalise the Mapping Account?

I have just argued that the mapping account, at least on its *prima facie* interpretation, is committed to the existence of abstract mathematical objects. However I am sceptical about the existence of such objects. There are of course the usual worries about how we can have knowledge of such objects, since if they exist they are non-spatial, non-temporal and acausal. Although some progress has been made towards solving some of the epistemological problems associated with abstract objects, as manifested in different research programmes, this does not dispel all the issues associated with such objects. For instance, Stewart Shapiro in his *Philosophy of Mathematics: Structure and Ontology* (1997) argues that the three methods of simple abstraction, linguistic abstraction and implicit definition are sufficient to give us knowledge of platonistic structures, but this relies on a certain amount of ideology, such as the principle of coherence, to guarantee the existence of the structures that are so defined (Ibid. p. 95). Moreover crucial issues of the identity conditions for structures remain unsolved.⁶⁶

Another influential research programme is Neo-Fregeanism, as manifested in the collection *The Reason's Proper Study*.⁶⁷ Neo-Fregeanism tries to analyse our knowledge of natural numbers in terms of principles such as Hume's principle: two concepts have the same number if and only if the objects falling under those concepts can be put into one to one correspondence. However a large issue for Neo-Fregeanism is explaining the epistemological status of such principles (ibid pp.11-12, 307-332). A related issue, the Caesar problem, also arises, whereby Hume's

⁶⁶ See the papers on structuralism in MacBride, F (Ed)(2006) *Identity and Modality*, Oxford: OUP.

⁶⁷ Hale, B and C. Wright (2001) *The Reason's Proper Study*, Oxford: OUP

principle cannot say of any object whether it is a number or not. (Ibid. p.14-16, 335-396). So despite recent work, serious problems with the possibility of our knowledge of abstract objects abound, and motivate the desire for an account of applicability that does not depend on such objects. For although the nature of the subject matter of mathematics is disputed, the brute fact of the applicability of mathematics to the empirical world is not, even though there is disagreement on how to explain this applicability.

If platonistic objects do not exist then the mapping account cannot, as it stands, explain how it is that platonistic mathematics facilitates the description and prediction of empirical phenomena. It would explain the applicability of abstract mathematical structures if abstract mathematical structures existed, but if they don't, it doesn't. So what now? There are two possible, but incompatible, nominalist responses. Firstly we could be fictionalists, declare that that abstract mathematical structures and abstract mathematical objects are only fictions, and then try to find some role for the mapping account as a theory about platonistic fictions. The use of these fictions to derive nominalistic results is legitimate (in the sense they cannot lead us astray) because (I will argue) the theories of these fictions are conservative over nominalist theories, and useful because of our ability (says Field) to construct abstract counterparts of nominalistic statements. It might be then wondered where this leaves the mapping account, but I do not want to go into detail here – the reader's curiosity will be satisfied shortly in 6.3.2, although the outcome is not a positive one ⁶⁸

There is a second nominalistic response to the extant platonistic commitments of the mapping account, which I alluded to above: we may try to

⁶⁸ It may be thought for some reason that the extensive role the discussion of Field played earlier in chapter four of this thesis commits me to the fictionalist argument. But this is not so, for there Field's ideas were merely used for providing intrinsic descriptions for the purpose of undermining belief in genuine platonistic explanations of empirical phenomena in order to add weight to the idea that mathematics is relevant to empirical phenomena only in a representational capacity. I intended no nominalist moral to be drawn from this exercise, and I only wanted to explore what the scope of the mapping account should be. As such my foregoing use of part of Field's philosophy has not in any way committed me to asserting any particular view about the ontology of mathematics.

reinterpret mathematics as being about something other than abstract objects.⁶⁹ If successful we still retain a mapping account but it is an account of the applicability of some nominalistically acceptable entities to empirical phenomena in terms of mappings from empirical structures to the nominalistically acceptable entity-structures rather than to abstract mathematical structures. If mathematics does not have to be about abstract objects then we may regard our nominalistic version of mathematics as mathematics in a serious sense, as *nominalistic* mathematics rather than *platonistic* mathematics. To motivate such a nominalist argument we would need to find surrogates for the natural and real number structures, sets and functions and the like, surrogates which are not abstract in the way that platonistic entities are and which are therefore nominalistically acceptable. The nominalistic structures would have to possess the same *structural* properties as the platonistic structures, but if the nominalistic structures are suitably rich that need not be a problem. Several versions of this second response are available and I describe two such versions here. One of these is developed by Davide Rizza (2010) – based on the results of Reinhard Niedersee (1992). The second is the modal-structuralist strategy of Geoffrey Hellman (1989).

6.3.1 A Scholarly Point about Rizza and Field.

Before I begin presenting the arguments outlined above, there is one point that I wish to take up. This is that although Rizza's attempted nominalisation is a clear example of surrogate nominalism since, as we shall see, he provides a nominalistically acceptable surrogate for real numbers *qua* c-types, he seems to take himself to be extending Field's programme. In fact he says as much:

[my] objective is to show that mathematical nominalism can be motivated as a methodological approach capable of providing valuable insight into the applicability of mathematics: the idea is to use mathematical nominalism as a basic theoretical outlook to investigate applicability and then to show that, from this starting point, a satisfactory

⁶⁹ This is to be understood along the lines of what Burgess and Rosen (1997) refer to as the 'modal' nominalistic strategy.

account of applicability can be developed. Such an objective is, to the best of my knowledge, not explicitly pursued by any recent work on nominalism in mathematics, although it is implicitly present in [Field, 1980]. (Rizza 2010 pp.56-57)

and even more explicitly:

What I want to show is that Field's explanation can be further refined and this can be done precisely by showing that it is unsatisfactory from the point of view of a mathematical nominalist. A deeper nominalization of Field's explanation produces a better account of applicability. (p.60)

It is true that Rizza's idea to use nominalism as a basic starting point to investigate applicability has not been explicitly pursued previously and that something like this is implicitly present in Field. What is not implicitly present in Field however, and in fact represents a departure from the Fieldian programme itself, is the idea that we need to nominalise measurement theory further than we have already done so, and by means of surrogates. If there were any dispute about this point, Field himself makes it very clear:

I do not propose to reinterpret any part of classical mathematics, instead I propose to show that the mathematics needed for application to the physical world does not include anything which even *prima facie* contains references to...abstract entities like numbers, function, or sets (Field1980 pp. i-ii)

For if we already believe that platonistic objects are useful fictions the invocation of which is conservative over nominalist science, and that the mapping account is not literally true, then why even bother to provide nominalistically acceptable surrogates for natural and real numbers, functions, and the like? Providing nominalistically acceptable surrogates for platonistic objects may appear to be a refinement or deeper nominalisation of Field insofar as reference to fictional abstract mathematical objects is avoided, but, from the Fieldian perspective, it is completely redundant and in conflict with Field's stated views. However this confusion is a purely scholarly issue, and since nothing about the validity of Rizza's claim to have nominalised much measurement theory is affected by it, I shall leave the discussion of it here, and turn to the first of the options for a non-platonistic construal of the mapping account, the fictionalist option.

6.3.2 The Fictionalist Approach to the Mapping Account

Before I can develop a fictionalist response to the mapping account it is essential to be clear regarding what fictionalism, specifically mathematical fictionalism, actually is. I said a little bit about fictionalism in chapter four, but I was primarily concerned with the part of Field's programme that focused on the possibility of producing nominalistic descriptions of platonistic-mathematics-containing empirical theories rather than with the part concerning fictionalism *per se*. Mathematical fictionalists believe that theories about platonistic objects are not literally true, but that nevertheless their use in inferring nominalistic consequences from nominalistic premises is both legitimate and useful, perhaps to the extent of being practically indispensable to such an enterprise. The reasons that fictionalists are nominalists is because they do not believe that abstract objects have any substantive existence, from which it follows that no statement about an abstract mathematical object can be literally true, though it can be 'true in the story of platonistic mathematics' – just as it is not literally true that 'Venus was the mother of Aeneas', since there is no such person as Venus and probably no Aeneas either, but this statement is true 'in the story of the *Aeneid*'. So far so good, but several questions immediately suggest themselves: (1) isn't there a risk that using platonistic mathematics will allow us to infer all kinds of consequences from nominalistic premises that would not follow from the premises alone? (2) can we actually give nominalistic versions of the premises of scientific theories that capture the empirical content of what the scientist is trying to express? and (3) if it is false, why should platonistic mathematics be useful at all?

Question 1: Accounting for the Legitimate Use of Platonistic Mathematics. The key concept in establishing that the fictionalist's use of platonistic mathematics is legitimate is *conservativeness*. One theory A is a conservative extension of theory B iff no consequences of a certain type follow from the conjunction of A and B that do not follow from B alone. Nothing said so far rules out that A and B might contradict each other, and if they are contradictory they certainly won't be conservative, since

all consequences whatsoever will follow from the conjunction. But if the language of one of the theories is restricted so that the contradiction cannot arise, then conservativeness can be preserved. As Field explains, we might think nominalistic and platonistic theories will be inconsistent, since platonist theories talk about objects that nominalistic theories explicitly repudiate, but this can be avoided by relativising all the quantifiers of each nominalistically-statable assertion in the nominalistic theory to quantify over only non-abstract entities, yielding a new theory which will lack the resources to express the proposition that abstract objects do not exist. This is done by introducing a predicate Mx (x is a mathematical object) and only permitting quantification over those objects x such that $\sim Mx$. In what follows the nominalistic theory containing the relativised quantifiers is denoted N^* .

The conservativeness of a theory with respect to another theory is differentiated by the order (first, second or higher) and type (semantic or syntactic) of the consequence relation they concern, as well as which category of consequences they involve. Thus we need to distinguish whether the consequence relation is semantic or syntactic since these are not coextensional in higher-order cases. A is *syntactically conservative* over B with respect to some class of formulae if no such formulae can be derived logically from $A+B$ that cannot be derived from B alone. A is *semantically conservative* over B if for all logical consequences of a certain class, no logical consequences of that class follow from $A+B$ that do not follow from B alone. In the first-order case A will be semantically conservative iff it is syntactically conservative. However in the second-order case this will not be true, since A might well be semantically conservative but will fail to be syntactically conservative, since second-order theories are syntactically incomplete.

Leaving aside for a moment such considerations, it is clear that for platonistic mathematics to be legitimately applied by the fictionalist to an empirical situation the platonistic mathematics must be conservative with respect to nominalistic consequences, that is, nominalistically conservative. A platonistic mathematical theory M is *nominalistically conservative* over a nominalistic theory N iff no nominalistic conclusions follow from $N+M$ that do not follow from N alone. Conservativeness may be stated in the form of principle C, where A^* and N^* are

nominalistic statements/theories that do not refer to abstract mathematical objects, even to say there are no such objects:

(C) Let A be any nominalistically statable assertion, and N any body of such assertions; and let M be any mathematical theory. Then A* isn't a consequence of $N^* + M + \exists x \sim M(x)$ unless A is a consequence of N^* alone. (Field 1980 p.12).

Just in case philosophers consider conservativeness to be a contentious proposition, Field shows that conservativeness is implied by other principles which have more intuitive plausibility. For example, the following principle implies C:

(C') Let A be any nominalistically statable assertion and let M be any mathematical theory. Then A* isn't a consequence of M unless A* is logically true. (Ibid.)

That is, from a theory purely about abstract objects the only statement about concrete objects that follows from it will be those logically true statements featuring concrete objects. It is evident that this principle *must* be true since all logical truths are logically valid formulae (tautologies), and all logically valid formulae follow from every theory. Thus if A* is a tautology then it will follow from every platonistic theory, even though all sentences of S will quantify over platonistic objects, and A* will not quantify over any such objects. If this philosophical argument were not enough to convince the conservativeness sceptic, we can prove formally that a given mathematical theory is conservative in the relevant sense. For example, in the case of Zermelo-Fraenkel set theory with urelements Field supplies the following proof:

Theorem: If T is any consistent body of assertions, then $ZFU_{V(T)}+T^*$ is also consistent.
Proof: Suppose T is consistent, then it has a model M of inaccessible cardinality, say with domain D. Pick any entity e not in D. (e is the empty set). Let D_0 be $D \cup \{e\}$; Let D_1 consist of all non-empty subsets of D_0 . Let D_2 consist of all non-empty subsets of $D_0 \cup D_1$, etc. Let D_ω be $D_0 \cup D_1 \cup \dots$. Let $D_{\omega+1}$ consist of all non-empty subsets of D_ω etc. Continuing in this way until you reach an inaccessible cardinal, you get, if certain initial precautions are taken on the choice of D and e, a model of $ZFU_{V(T)}+T^*$. So $ZFU_{V(T)}+T^*$ is consistent. QED. (Field 1980, pp.17-18)

This argument will not persuade a nominalist conservativeness-sceptic of the validity of conservativeness, since it proves the conservativeness of set theory by

using set theory. However, the informal argument given above for the truth of C' will hopefully be enough to convince the nominalist that platonistic mathematics is conservative over nominalist theories. Indeed, as presented, this view may even seem trivial, assuming the mathematics is consistent. Field explains:

This argument isn't conclusive: standard [i.e. platonistic] mathematics *might* turn out not to be conservative...for it might conceivably turn out to be inconsistent, and if it is inconsistent is certainly isn't conservative. We would however regard a proof that standard mathematics wasn't consistent as extremely surprising, and as showing that standard mathematics needed revision. Equally it would be surprising if standard mathematics implied that there are at least 10^6 non-mathematical objects in the universe...Good mathematics is conservative; a discovery that accepted mathematics isn't conservative would be a discovery that it isn't good. (Field 1980, p.13)

Is platonistic mathematics conservative in the case of mixed mathematical statements, e.g. those involving impure sets, functions from empirical objects to mathematical objects etc? For in science in general, and physics in particular, the relation between N and M is far from obviously arbitrary because platonistic mathematics saturates the physical sciences: many concepts are defined with the help of such mathematics, some have only a platonistic interpretation or definition, and platonistic mathematical methods – operations on differential equations for example – are seemingly vital to scientific work. Conservativeness seems to be by no means a trivial property in such cases. So how can it be shown that platonistic mathematics is conservative here?

Field's response is to avoid such questions by explicitly constructing *non-platonistic*, that is, nominalistically acceptable, versions of scientific theories, such that principle C above will apply trivially to them. It is thus necessary to show that there *can* be nominalist versions of scientific theories, a theory N* that does not contain any references to platonistic objects, and that the platonistic versions of them obtain if and only if the nominalistic ones also do.

Question 2: Producing Nominalistic Theories. Producing nominalistic scientific theories is essential to the fictionalist for several reasons. One reason, as Field explains, is that nominalistic theories don't rely on arbitrary entities such as

numbers. He commends Hilbert's approach to geometry for avoiding arbitrariness and wishes to emulate it. (Field 1980 p.46). It has been suggested (e.g. MacBride 1999, p.434) that "nominalist theories invoke only entities causally relevant to what is being explained" but it is doubtful that Field wishes to go this far since it is not clear how the fact a set of space-time points with the right properties can model the real numbers involves causal relevance.⁷⁰ The points may perhaps be accessible and even have causal powers, but this seems irrelevant to the role that they play in a nominalistic version of a platonistic theory. It could also lumber Field with the possibility that his nominalisation could collapse if space-time turned out to be quantised. There is unfortunately no space to address these fascinating issues here.

The second reason that fictionalists need nominalised theories is to ensure that the conservativeness of mathematics over nominalist science is a clear-cut matter, and thus that the fictionalist's use of platonistic mathematics remains legitimate. For if the theories are nominalised (and their quantifiers restricted in the manner outlined above) it is clear that platonistic mathematics *has* to be conservative over them. This is exactly what Field sets out to produce in *Science without Numbers*, specifically for the case of Newtonian gravitational theory. I discussed this at length in chapter four, so shall say no more about the matter here for fear of repeating myself, but request the reader's continued indulgence concerning both the view that nominalising scientific theories in general is a plausible and feasible enterprise and the view that Field has successfully nominalised Newtonian gravitational theory.

Question 3: Abstract Counterparts and the Usefulness of Platonistic Mathematics. So we know, granting the answers to the first two questions, that there are nominalistic versions of scientific theories and thus that the platonistic mathematics used by the fictionalist is conservative over nominalist theories and can therefore be used legitimately. This has not however explained why the

⁷⁰ The fact that Field (as quoted earlier) says that nominalist theories do not involve causally irrelevant entities does not imply that he feels the entities invoked in his nominalistic theory are causally relevant in a strong sense.

platonistic mathematics is *useful* in the derivation of nominalist consequences of nominalistic physical theories rather than merely *harmless*. Field does have a clear line on why this should be the case:

I think the key to using a mathematical system [M] as an aid to drawing conclusions from a nominalistic system N lies in proving in $N^* + [M]$ the equivalence of a statement in N^* alone with some other statement (which I'll call an *abstract counterpart* of the statement in N^*) which quantifies over abstract entities. (Field 1980 p.20)

The reason the abstract counterparts are useful is because

...if we want to determine the validity of an inference in N^* ...it is unnecessary to proceed directly; instead we can if it is convenient 'ascend' from one or more statements in N^* to abstract counterparts of them, then use [M] to prove from these abstract counterparts an abstract counterpart of some other statement in N^* . (Field 1980 pp.20-21)

What we need to prove therefore is that for each nominalistic premise n_i of N^* , n_i iff m_i , where m_i contains references to abstract mathematical objects. Thus when we get the conclusion involving abstract counterparts (m_k) of the derivation from $N^* + M$, we can prove that m_k iff n_k and thus that n_k . That in a nutshell is how platonistic mathematics can be used – because M is a conservative extension of N we literally cannot go wrong with the platonistic mathematics if we do the derivations correctly, assuming of course that M is consistent.

This explains *how* M can be used to facilitate derivations in N. But *why* should we want to use it? This was addressed in chapter two, but to reiterate, because abstracting away from the content of the nominalist statements means that many of their details can be ignored. This makes the abstract statements easier to understand and grasp than their nominalistic counterparts, even though strictly speaking they are false as they quantify over objects that do not exist. The fact that they ignore many details relevant to the nominalistic statement means that the derivation of an abstract statement is simpler and faster than the derivation of a nominalistic one. We can therefore derive empirical predictions that could be derived without using platonistic mathematics but which would take much longer in such a situation. Field presents a useful example of this in the case of counting, a

clear application of platonistic mathematics insofar as numbers and sets are involved. One way to present cardinality statements nominalistically is to state the cardinality logically in the usual way. It is convenient to introduce special quantifiers for larger collections of objects e.g. $\exists_2 x$ for $(\exists x)(\exists y)(Fx \ \& \ Fy \ \& \ (z)(Fz \supset z = x \vee z = y))$ although here 2 is not a numeral, the name of a number, but merely an abbreviation. Suppose says Field, we are told, nominalistically, the following:

1. There are exactly 21 aardvarks $(\exists_{21}x)Ax$
2. On each aardvark is three bugs $(\forall x)(\exists_3y)(Ax \ \& \ By \ \& \ yOx)$
3. Each bug is on one aardvark $(\forall y)(By \supset (\exists_1x)(Ax \ \& \ yOx))$

and we want to know how many bugs there are. The answer – $(\exists_{63}y)By$ – can be derived purely logically but it is extremely unwieldy, and impractical to demonstrate here. However with some platonistic mathematics the proof of this is extremely simple. (Field 1980 p.22). We can state 1-3 platonistically as follows:

- 1* The cardinality of the set of aardvarks is 21.
- 2* All sets in the range of the function whose domain is the set of aardvarks, and which assigns to each entity in its domain the set of bugs on that entity, have cardinality 3.
- 3* The function mentioned in 2* is 1-1 and its range forms a partition of the set of all bugs.

And together with the three following platonistic statements:

- a. If all members of a partition X of a set X have cardinality a and the cardinality of the set of members of the partition is b then the cardinality of X is $a \cdot b$
- b. The range and domain of a 1-1 function have the same cardinality
- c. $3 \cdot 21 = 63$

we can infer directly that:

4* The cardinality of the set of all bugs is 63

Because the abstract counterpart holds iff its corresponding nominalistic statement holds then 4* iff 4. That is 'The cardinality of the set of all bugs is 63' iff $(\exists_{63}y)By$, and by simplification and modus ponens, $(\exists_{63}y)By$. The proof that in logic was too long to include in this thesis was converted into a few lines with the help of platonistic mathematics, although that mathematics was theoretically dispensable as it did nothing that the logic could not do, it just did it more quickly. The fictionalist has therefore shown, I submit, how platonistic mathematics may be useful despite being false.

This explains how it is that the fictionalist can have his platonistic cake and eat it too, but where does this leave the mapping account? The primary use of that account is by platonists wishing to explain how platonistic mathematics can be usefully applied to the empirical world. Its primary use for fictionalists is to convince platonists that the indispensability argument is unsound. This would not of course affect those platonists who were not platonists because of that indispensability argument, but for other reasons. Indeed they may even be grateful to Field for supplying them with an account of applicability, whilst his criticism of their platonism bypassed them entirely. The fictionalist of course does not need to explain how platonistic mathematics can be applied since he does not believe in the existence of abstract mathematical objects, and he can explain the applicability of mathematics, despite the non-existence of its supposed subject matter, in terms of conservativeness and the simplification of inferences.

The question is this: does the mapping account have no use for the fictionalist apart from the undermining of the indispensability argument? Insofar as the account explains the applicability of something the fictionalist views as a fiction, it might be thought that the mapping account is as useful to the fictionalist account of the applicability of mathematics as explanations of the technology in the fictional *Star Trek* universe are to the show, i.e. an important part of the show but ultimately dispensable to the actual world we live in. Is this so with the mapping account? This

issue is tricky. We can't just regard the mapping account as a conservative extension of a nominalist theory, since unlike, say, number theory, it involves substantive non-mathematical claims, about what properties empirical structures have to satisfy to be represented, how idealisations are best dealt with and the nature of laws. At best the fictionalist can say 'if there were such a thing as abstract mathematical objects, the mapping account would explain how we can apply them', whilst continuing to use the account to the disadvantage those who believe in such objects on the grounds of their putative indispensability to the sciences.

There is however another option for the nominalist who feels that there is some truth in the mapping account. This option is to reinterpret platonistic mathematics to be about something other than abstract mathematical objects, to find nominalistically acceptable surrogates for abstract mathematical objects and structures. If this is possible then the mapping account will likely be able to explain correctly how and why a nominalistically acceptable version of (parts of) platonistic mathematics can be usefully (and truly) applied to the empirical world, still in terms of mappings of course. It will be a mapping account of the applicability of nominalistically acceptable structures to empirical phenomena, rather than an account of the applicability of platonistic structures to empirical phenomena, but it will be a mapping account of applicability none the less. Such an account would be required by this sort of nominalist, since he cannot appeal to conservativeness to do the work: insofar as nominalist reinterpretations of platonistic mathematics are about concrete objects, the claims made by a nominalised mathematical theory N will not be conservative extensions of a nominalistic scientific theories T , rather $T + N$ will have more nominalist consequences than T alone. Of course the fictionalist is free to believe that platonistic mathematics is a useful fiction, but agree that the form of the mapping account discussed below explains the applicability of nominalistically acceptable structures to the empirical world, assuming that those structures exist and the statement of this theory is nominalistically acceptable.

6.3.3 Nominalistically Acceptable Surrogates I: Rizza's Approach

One philosopher, and to my knowledge the only philosopher, who has both drawn attention to the fact that the mapping account as usually understood contains measurement theory committed to platonistic mathematics *and* suggested a way to avoid this along the lines of a surrogate-nominalist approach, is Davide Rizza, in his 'Mathematical Nominalism and Measurement' (2010), which provides a sketch of how it may be possible to nominalise the measurement theory that appears in the mapping account and why it would be desirable to do so. Rizza explains that his approach differs from that of some other contemporary nominalists:

As a matter of fact, the recent philosophical literature provides many different ways of dealing with measurement statements in a nominalistic fashion; so it would seem sufficient simply to choose one of them in order to obtain the result I am aiming for. However, the particular philosophical project I pursue in this paper requires a nominalistic approach which is an alternative to the existing ones. (Rizza 2010 pp.56)

Rizza's strategy is to use the results of some foundational work in the theory of measurement carried out by the mathematician Reinhard Nedderee.

What needs to be nominalised? Before we march straight into the nominalisation suggested by Rizza, I want to spell out exactly what the *prima facie* platonistic commitments of this measurement theory, and *a fortiori* the mapping account, are. For if the mapping account is to be nominalised we need to nominalise not just the measurement structures, etc, but also the apparatus involved in proving the representation theorems. Take a simple case such as that of extensive measurement as explored in chapter two. We have an empirical structure $\langle A, \succeq, \circ \rangle$ which contains a set A of physical objects each with a certain magnitude of some attribute. We also have a platonistic structure $\langle R, \succeq, + \rangle$ which contains a set, the real numbers, and mathematical relations/operators \succeq and $+$, the latter taking pairs of real numbers to other real numbers. Additionally there is an homomorphism function ϕ taking elements of A to elements of R such that additivity and weak order are preserved.

To turn this into a system of measurement we need to impose further restrictions on the empirical structure, along the lines of chapter two. That is, we need the following axiom system for any $x, y, z \in A$:

1. Weak order: (empirical relation \geq is transitive and connected)
2. Concatenation is associative: $(\forall x)(\forall y)[(x \circ (y \circ z)) \sim ((x \circ y) \circ z)]$
3. Monotonicity: $(\forall x)(\forall y)(\forall z)[(x \geq y) \equiv ((x \circ z) \geq (y \circ z)) \equiv ((z \circ x) \geq (z \circ y))]$
4. Archimedean axiom: $(\forall x)(\forall y)(\exists n)(nx \geq y)$.⁷¹

We can see that whilst most of these axioms are purely empirical, the Archimedean axiom does not appear to be empirical since it quantifies over natural numbers n . Thus we have four platonistic elements in even this simple piece of measurement theory, namely sets, real numbers, natural numbers, and functions.

Nominalising certain sets. At least one of these references is easy to eliminate, namely the set A . It seems to me that the treatment of A as a set is merely a convenience, and that nothing hinges on A being a *set* rather than just a plurality of elements (there are no sets in the transitive closure of A) and so we are perfectly at liberty to treat A as a plurality that consists of nothing over and above its members, which are empirical objects. The other references are not so amenable to such a quick treatment. For example, how are we to avoid references to the real numbers (and their associated relations and operations of $>$ and $+$)?

Nominalising values-of-measurement. Rizza (and Niederee) do have a suggestion for how to use something other than real numbers as values for measurement. This suggestion involves using some entities known as c-types as such values. The types in question arise from the fact that when we iterate m times some unit a of a

⁷¹ This form of the axiom differs from the more complex version of the axiom given earlier in chapter two, but this is fine since any form of the Archimedean axiom that quantifies over natural numbers will do illustrate the point I am trying to make here.

physical property, one of the following will obtain with respect to any magnitude x of that physical property which is iterated n times (Rizza 2010, p.66):

$$(A)(ma > nx) \text{ or } (B)(ma \sim nx) \text{ or } (C)(ma < nx)$$

Each of A, B and C gives rise to A, B and C types, each respectively the collection of all atomic formulae such that A, B or C holds when some empirical magnitude b is the value of x . Rizza only needs the c-types to motivate his view. The c-type $C(a, b)$ is the class of all atomic formulae such that $ma < nx$ where b is substituted for x . What this means is that for every possible assignment of a physical magnitude b to x such that the inequality is satisfied we will get a c-type $C(a, b)$. We could just as well have used a-types or b-types, but they are all equivalent, and as Rizza points out c-types have the advantage that “the class [collection] of all inequalities of the form of (C) determines all approximations of x in a units from below and thus it can be used as the measure of x ” (Rizza. 2010 p.66). Of course, so can the other types approximate x , but approximating ‘from below’ seems to fit better with our intuitive idea of getting more and more refined measurements as we approach up to some given magnitude – or at least I take it that this is Rizza’s reasoning.

So these are c-types, but what use are they in measuring, or being values of measurement? The answer is that “... $C(a, b)$ identifies a converging sequence of approximations of b to a : then a positive real as the limit of a sequence of approximations, may be identified with... $C(a, b)$ itself...” (Rizza 2010 p.66). So each c-type $C(a, b)$ will be a point on a continuous scale, a value of measurement. This procedure makes measurement yield c-types rather than real numbers, and the c-types function like reals do, as continuous values of measurement, which are, Rizza contends, not abstract objects in the way that real numbers are as c-types are collections of formulae. There is no reason to regard the collections in question as abstract objects such as sets rather than just pluralities of formulae, and no *prima facie* reason to regard the formulae themselves as abstract, for they are, as I make clear in a few pages, formula tokens. (In the subsection below, ‘are c-types nominalistically acceptable?’, we see that there is a little more to this issue.) Rizza’s

idea with respect to the real numbers is that “the positive reals with order and addition can be understood as a system of measures generated by [an empirical structure]” (Rizza 2010, p.65), since they are identified with c-types, which are actually a system of measures generated by the empirical structure in question. I shall discuss whether or not c-types are really nominalistically acceptable below, for now I merely want to present Rizza’s views. The idea that there can be other values of measurement than just the reals is not an idea that was first developed by Rizza or even Nedderer, indeed it appears to be reasonably commonplace even as early as the nineteen sixties, by the time measurement theory was an acknowledged discipline. The measurement theorist Ernest Adams writes:

It seems to me that in characterising measurement as the assignment of numbers to objects according to rules, the proponents of the representational theory have fastened on to something which is undoubtedly of great importance in modern science, but which it not by any means an *essential* feature of measurement. What is important is that the real numbers provide a very sophisticated and convenient conceptual framework which can be employed in *describing* the results of making measurements: but what can be conveniently described with numbers can be less conveniently described in other ways, and these alternative descriptions no less ‘give the measure’ of a thing than do numerical descriptions. Thus, the hardness of a mineral specimen may be described as 7 on the Mohs scale, but alternatively as of the same hardness as quartz. (Adams 1966, p.129)

Nominalising functions. The use of c-types bodes well for the nominalist project of eliminating the real numbers from measurement, but it says nothing about the third use of platonistic mathematics in our simple measurement theory example, concerning functions involved in measurement, e.g. in the mapping of an empirical structure into an abstract mathematical structure. Rizza maintains “the function $[\phi]$ relating an empirical structure...to its measuring structure is itself defined by a procedure to compare multiples of physical objects in M ; its action is entirely determined by empirical facts” (Rizza 2010 p.67). This contention is very plausible, because empirical magnitudes are assigned to c-types on the basis of an empirical comparison operation, and both the empirical structure and the c-types are (supposedly) empirical. Platonistic mathematics would only become involved if we

showed that such an empirical structure has a certain relation to an abstract mathematical structure. But by utilising the (arguably nominalistically acceptable) c-types we don't need to have a function from empirical structures to platonistic structures, but rather from empirical structures to empirical structures, so there is nothing platonistic going on. It might be objected here that although we may have empirical structures both sides of the mapping, the mapping *itself*, as a function, is a platonistic object. The cogency of this response depends on whether a function *is* an irredeemably platonistic object. But why should we think this? A function is an assignment of an object to another object based on some rule. If I pair up, say, knives with forks, so that there is a 1-1 correspondence between knives and forks, there is no reason to think this is a platonistic operation. It can of course be *described* platonistically, but then so can baseball, and no one believe that baseball is an example of platonistic mathematics. Because a function can be described platonistically does not mean that it *is* a platonistic function. So Rizza is correct that there is nothing irredeemably platonistic going on here, *assuming that c-types are not abstract objects*.

Nominalising the Archimedean axiom. The fourth piece of platonistic mathematics in the simple example above was the Archimedean axiom. This axiom is *prima facie* nominalistically troubling as it quantifies over natural numbers in its statement $(\forall x)(\forall y)(\exists n)(nx \geq y)$. It cannot simply be eliminated from the axiom set however since it is a necessary axiom. It is suggested by my argument above in chapters three and four that no reference to any abstract mathematical object is essential for any physical theory. Rizza agrees. But what could the empirical content of the axiom be? It must be that 'all empirical magnitudes are comparable'. It might be thought not a straightforward matter that this is an empirical claim, as it effectively says that any empirical magnitude can be made greater than another empirical magnitude by concatenating it to itself a certain *number* of times, and *numbers* are not empirical objects. I do not think that this is problematic however, given the inductive definition from chapter two for nx , namely $1x = x$, $2x = 1x \circ x$, ... , $nx = (n-1)x \circ x$. The numbers in the definition are nothing more than abbreviations

for iterated concatenations. So it seems what the Archimedean axiom is really saying is that for any two empirical magnitudes x and y such that $y \geq x$, if you keep concatenating x with itself, $x_1 \circ x_2 \circ \dots x_k$, you will eventually make it the case that for the concatenation kx , $kx \geq y$. It seems that the platonistic version of the Archimedean axiom captures abstractly the empirical content of the axiom, but that it would be a mistake to think that numbers are what that axiom (as it concerns empirical magnitudes) is *really about*. The version of the axiom involving natural numbers, the abstract version, is a way of stating the empirical content of the axiom, not the other way around. Of course the same cannot be said for the version of the axiom that is about real numbers – in that version of the axiom, numbers, natural and real, are what the axiom is about, although I have contended that such entities do not really exist.

Would Rizza agree with me regarding the status of the Archimedean axiom? Initially it seems that Rizza's formulation of the content of the axiom is very different to mine, since he asserts "Archimedes' axiom is equivalent to discriminability by atomic formulas" (Rizza 2010, p.69), and nowhere do I mention discriminability or atomic formulae. However, Rizza explains that this is in fact another way of saying "Archimedes' axiom is equivalent to the adequacy of an empirical procedure exclusively based on concatenations of objects" (Rizza 2010, p.69) which is how I have characterised the axiom myself, with an inductive definition of n . So why does Rizza not straightforwardly accept the inductive definition, which surely captures the empirical content of the axiom, rather than using his more convoluted formulation in reaching seemingly the same conclusion?

The reason has to be that he does not want a piecemeal approach to nominalisation – in linking his statement of the Archimedean axiom to the notion of c-types that also provide his surrogate values-of-measurement he avoids having a nominalist surrogate for the real numbers here, an unrelated surrogate for the natural numbers there, etc, rather he makes the notion of a c-type central and thus imposes a certain amount of unity (though not necessarily simplicity) on what could otherwise be a disparate nominalistic enterprise. Indeed he says "...as a

consequence of [c-types]...Archimedes' axiom can be reformulated as a purely empirical condition" (Rizza, 2010 p.65). Clearly this empirical condition is that if an object exhibits a lesser magnitude of an extensive attribute F than another object then that object can always be concatenated to itself enough times that the resulting composite object will be greater with respect to F than the other object.

Are c-types nominalistically acceptable? I suggest that, utilising the work of Rizza and Nedder, we have seen that four examples of cases where platonistic mathematics is used in the mapping account can be in fact understood without any platonistic concepts, or at least without reference to any platonistic objects, at all, or so I have asserted – so long as c-types are nominalistically acceptable. Are they? That question depends on what counts as nominalistically acceptable and what a c-type actually is. I am going to follow Field and carve the what is nominalistically acceptable from what isn't along the lines of abstractness, rather than along lines of cardinality or anything else. So an object x is nominalistically acceptable just in case it is not abstract. A c-type as we have said is 'a class of all atomic formulae such that $ma < nx$ where b is substituted for x ', where ' $C(a, b)$ identifies a converging sequence of approximations of b to a ...'. Since classes here can be eliminated in favour of pluralities we can instead refer to c-types as pluralities of atomic formulae. And there is no way anyone could construe tokens of atomic formulae as anything other than concrete, assuming it is formulae tokens rather than formulae types that are intended, which does seem to be what is meant, as they are generated by actual concrete acts of measurement. But what of the relation of c-types to limits of sequences, which are normally viewed as abstract mathematical objects? Again this of no matter, for Rizza did not say that c-types *had* to be identified with limits of sequences, i.e. are such limits, merely that they *can* be so identified, suggesting that this identification could take place if such limits existed, if we wanted to talk about positive real numbers rather than c-types. If such limits could be nominalised there would be even less cause for concern.

Another more serious issue is that there are clearly not enough actual c-types to yield all values of measurement, since there could not be an uncountable number

of acts of measurement. To give an entire continuum of possible values of measurement something other than actual c-types must be introduced. The obvious suggestion is *possible* c-types, which would yield a scale of measurement consisting of both actual and possible c-types. These would be collections of possible formula tokens. But are possible c-types nominalistically acceptable? There is nothing unacceptable about possibility in general, for instance if I say it is possible that my house will be struck by a meteor. But if I were to say my house might be struck by a possible meteor that is a different story. And it seems as if using possible c-types as values of measurement is an example of the latter, talk of possible objects rather than the possibility of objects. There is an extensive literature on the status of *possibilia* and I do not wish to go into the issue here, but if (some) c-types are *possibilia* and *possibilia* are abstract objects then this poses a serious problem for the nominalistic acceptableness of Rizza's c-type account. Moreover this would mean that there is much less to distinguish Rizza's account from the modal-structuralist account to be considered now, since both accounts would be fundamentally modal, and there would be less to make Rizza's a *sui generis* nominalistic account of applicability, as he has claimed.

6.3.4 Nominalistically Acceptable Surrogates II: Hellman's Approach

C-types however by no means offer the only surrogates for real numbers which could be utilised by a philosopher wishing preserve the mapping account in some form but eliminate its reference to abstract objects. At least one other nominalist programme provides an alternative. For example, Geoffrey Hellman's modal structuralism suggests replacing both natural and real numbers by *positions in possible concrete structures*.⁷² With respect to truth claims about natural numbers:

⁷² Charles Chihara also has a nominalist philosophy that can give a nominalist treatment of the real numbers. However I intend only to show that the programme of removing commitments to abstracta from the mapping account of the mapping account does not stand or fall with acceptableness of c-types, so I have omitted a discussion of Chihara here.

on the platonist view, ‘truth’ means ‘truth in the standard model’, either a unique model of the natural numbers or a fixed set-theoretical model...[but]...on the modal structuralist view, ‘truth’ means roughly ‘true in any possible model [of the natural numbers]’ where this is spelled out as truth of the relevant counterfactuals. (Hellman 1989 p.34).

Mutatis mutandis for the real numbers. This notion of truth arises naturally out of the modal-structuralist interpretation of mathematics, which consists of a *hypothetical* component and a *categorical* component. The hypothetical component consists of the re-interpretation of platonistic statements about a particular theory, e.g. number theory or real analysis. These statements are reinterpreted in a schema of the following form:

$$\Box \forall X \forall R [\wedge PA^2 \supset A]^X (R/S) [A_{\text{msi}} \text{ schema}].$$

That is, necessarily, for all classes X and relations R , the conjunction of the second-order Peano axioms implies A , where the quantifiers in the Peano axioms and A are relativised to class X . Furthermore, R replaces S in the Peano axioms and A , where S is the successor relation and R is an arbitrary unary relation. As an example of a modal-structuralist interpretation consider the commutativity of addition of the natural numbers, $a + b = b + a$. The modal-structuralist interpretation of this would be: $\Box \forall X \forall R [\wedge PA^2 \supset \forall x \forall y (x+y = y+x)]^X (R/S)$, that is, commutativity of $+$ follows from the conjunction of the Peano axioms relative to all those possible structures which satisfy the axioms. The point of R/S here is of course to get across that it is the structure of the sequences we are interested in, not particular elements of sequences or particular relations on them. Thus rather than the ‘ S ’, designating a particular successor function, any unary R which yields the intended structure will suffice. The background logic is second-order logic plus system S-5 of modal predicate logic.⁷³ Incidentally, the notion of necessity used, and denoted by ‘ \Box ’, is ‘ A

⁷³ For more information see e.g. Hughes and Cresswell (1996) chapter 3. One note though – the modal logic the modal-structuralist uses must not contain the Barcan formula ‘ $\Diamond \exists x Fx \supset \exists x \Diamond Fx$ ’ as an axiom, for although the modal-structuralist wishes to talk about the possible existence of structures he does not want to talk about the existence of possible structures! Such possibilities are nominalistically unacceptable to the modal structuralist.

holds “in any progression [ω -sequence] there might be, logically speaking” (Hellman 2005 p.553), rather than the more platonistic ‘necessarily, for all ω -sequences x , A ’. Similar sentences exist for real numbers – a consequence follows from the conjunction of the axioms of real analysis, relativised to all classes X which have a structure that satisfies those axioms. There may well be a variety of such classes but that doesn’t matter for the modal structuralist as is clear from the hypothetical sentence: any structure in the class of such structures will suffice.

In order to avoid vacuity arising from the non-existence of ordinal and continuous sequences, and nominalistic versions of arithmetic and analysis being true of nothing at all, modal-structuralism must take it as an axiom that ordinal and continuous sequences are possible, and if they are possible then such-and-such a fact about them will be true. Thus modal-structuralism contains a categorical as well as hypothetical component, with a categorical axiom for each type of structure being studied. The categorical axiom with respect to real analysis is:

$$\Diamond \exists X \exists R [\wedge R A^2]^X (R/<)$$

‘it is possible that there is a continuous sequence’

The X in the above categorical proposition ranges over ‘complete, separable, ordered continua’. These continua are structures that satisfy the following three axioms:

- (1) Order: ‘>’ is a dense linear ordering without endpoints.
 - (2) Separability: There exists a denumerable dense subset
 - (3) Continuity: Every non-empty bounded subset has a least upper bound
- (Hellman 1989b p.321)

Whereas in the case of progressions, possible concrete marks can be used as a model of the Peano axioms, a potential *infinity*, so Hellman invokes concrete objects as a basis for potential *continuity*. These concrete objects are space-time points. Hellman succinctly states his position:

...as long as one works within RA^2 one can conceive of its entities as physical in the sense of space-time physics in its usual formulations. That is, a manifold of space-time points can be taken as the ground-level objects (actual or hypothetical) of an RA^2 model (via a coding of \mathbf{R}^4 into \mathbf{R})⁷⁴, with space-time regions as the second-order objects (making up the range of the monadic second-order variables) (Hellman 1989 p.325)

I explained in chapter four that the nominalistic acceptableness of space-time points has been queried by some philosophers, but I maintained their nominalistic acceptability nonetheless. Thus we see that both Field and Hellman have a shared basis for their nominalisations, namely the use of physically real continua, which may offer an advantage over Rizza's theory in terms of parsimony – if you believe in space-time points then why invoke c-types if you only need to invoke space-time points? Hellman spends some time addressing the issues of the points of his continua as values of measurement: take for example, mass. The mass m of an object x is usually, as was made clear in chapter two, given in the form of a function whose value is a real number r i.e. $m(x) = ra$. This real number indicates how much of a certain unit the mass of x is. Thus the mass of an apple might be measured as 0.35 of a kilo. As with the above example of counting, this function has a modal-structural interpretation viz:

$$\Box \forall X \forall R [(\wedge RA^2)^X (R/\prec) \supset \exists F (F \text{ “represents mass”} \ \& \ F(x) = r \text{ in } X)].$$

- (i) F takes on (within limits of experimental accuracy) all actually measured values experimentally determined as values of mass;
- (ii) F agrees (within experimental limits) with all theoretically predicted values-of-mass under real world conditions) (Hellman 1989a p.102)

Note the expression ‘ F represents mass’ is Hellman's, not mine, and indicates a given unit of mass. Again, in English, this reads something like ‘necessarily for all complete, separable, ordered continua X , there is a unit of mass (e.g. the kilogram) and the value of the mass of x in that unit is given by some object in the continuum’. Clearly the usual axioms have to hold so that the measurement system in is appropriate for the extensive measurement of mass, and ensures any continuum forms a suitable measurement scale, but this adds no extra conceptual difficulty.

⁷⁴ \mathbf{R}^4 refers to ordered quadruples of real numbers.

Thus modal-structuralism does appear to have quite plausible and natural interpretations of real measurement. Indeed this approach to values of measurement seems to me to be at least as natural as Rizza's, and so we see that there are a variety of possible approaches for those philosophers who wish to remove references to abstract mathematical objects from the mapping account. In fact, given that it seems that c-types involve possible objects (infinite sequences) just as Hellman's does, Hellman's version may be preferred. For at the very least it has no real ontological commitments that Rizza's does not, gives a uniform treatment of arithmetic and real analysis, avoids talking about c-types, and is somewhat simpler in some of its essentials. Such an approach is certainly one avenue for the philosopher who is convinced that the mapping account is the right way to think about applicability, but wants to dispense with references to the nominalistically unacceptable abstract objects. I submit then that there is at least one nominalist construal of the mapping account of applicability, whether it depend on a fictionalist approach or a surrogate-nominalist approach.

6.4 The Role of Idealisations.

There have been several opportunities thus far to discuss the place of idealisations in a mapping account of applicability. I could have done so in chapter two, when the mapping account was developed and the place of partial isomorphisms was discussed. Or in chapter three where I considered platonistic explanations of empirical phenomena. However I decided to wait until now because I wanted to present my views on idealisation in a single place, rather than them being scattered throughout this thesis. My first aim in this section is to provide a taxonomy of the varieties of idealisation. Following this I shall investigate in some detail Batterman's claim that there are many idealisations to which the mapping account cannot do justice. I shall undermine his argument by criticising the consequences he draws from his example, but despite this I shall also consider the implications his example

would have for the mapping account if the example did in fact do the work that Batterman feels does. Following this, I shall look at how idealisations can be accommodated by the user of the mapping account, and discuss how this applies to the idealisations used in the work of both Field and Rizza.

6.4.1 Categorising Idealisations

There are many, many idealisations used in physical science. Penelope Maddy names several of them in her monograph *Naturalism in Mathematics*:

[f]or example, we treat a section of the Earth's surface as flat, rather than curved, when we compute trajectories; we assume the ocean to be infinitely deep when we analyse the waves on its surface; we use continuous functions to represent quantities like energy, charge and angular momentum, which we know to be quantised; we take liquids to be continuous substances in fluid dynamics, despite atomic theory. (Maddy 1997 p.143).

This list provides us with a decent sample of idealisations from which to begin an analysis. The most important issue that has to be settled is whether the idealisations in question are physical or not. Although I criticise a putative example of a non-physical idealisation below (which Batterman refers to as a 'mathematical' idealisation, a term I shall retain since it has entered the literature now) and am not especially sympathetic to the concept on some construals of it, it does not follow that I can reject such idealisations out of hand. However I do believe that for the most part idealisations are physical, for they generally involve simplifying physical assumptions. Making a physical idealisation involves deliberately mis-describing a physical situation for the purposes of simplifying statements about that situation within a given margin of error, and as such may involve a description of the system in which certain elements are removed or abstracted away, or a description of the situation that has a different structure than that which the descriptandum has in reality. It is evident that the majority of Maddy's examples comprise physical idealisations where the structure of the system is changed, for example viewing fluids and quantized quantities as continuous, and water as infinitely deep. These examples are not just examples of a specific quantity of empirical objects being

imagined to be added to the system to make it ideal – the difference between a discrete and continuous quantity may be one of cardinality but the former cannot be turned into the latter by the successive addition of elements, as anyone familiar with basic set theory will know. Rather such idealisations involve a deliberately false redescription of the structure of the system itself.

Instances of idealisations that consist in *removing* elements from a system to make it easier to describe are perhaps simplest. Suppose we want to calculate how many people it takes to pull a stone block. To simplify the calculation but still obtain a usable result I may try to eliminate some of the forces involved if certain conditions are met, even though such forces have *some* impact on this quantity. For example, suppose the block is extremely smooth and being pulled along a smooth and lubricated surface such that the friction is not a significant factor in the consideration of how many people need to pull the block. I may then decide in my calculations to treat the block as if friction is not one of the forces acting upon it, and thereby perform a vector calculation with fewer forces, a simpler calculation. Clearly this is a *physical* idealisation as I have given a physical description of a physical situation in such a way as to facilitate simplifying that situation.

One question that will present itself to the reader will surely be ‘do these two categories (abstraction/redescription) exhaust all physical idealisations?’. It is not necessary to my purposes to digress and consider this question here, though I think an affirmative answer is by no means unlikely. Far more pressing question is ‘must all idealisations used in physical theory be physical idealisations?’ and if not ‘what is the explanatory role of non-physical idealisations?’. In a recent and provocative paper ‘On the Explanatory Role of Mathematics in Empirical Science’, Robert Batterman has defended the existence of non-physical idealisations, which he calls *mathematical* idealisations, and tried “to account for how mathematical idealisations can have a role in physical explanation” (Batterman 2010 p.2). It might be thought at first that Batterman is simply using the phrase ‘mathematical idealisation’ as ellipsis for ‘physical idealisations in applied mathematics’, but this cannot be the case since his view, as we will see below, is that the mapping account is inadequate in some sense because it cannot deal with the possibility, which

Batterman believes to be realised, of mathematical operations explaining physical phenomena, or idealisations based upon mathematical operations playing a significant role in physical explanations. If by ‘mathematical idealisation’ he simply meant a physical idealisation in applied mathematics then this ambition would be redundant, as Batterman admits that mapping account can handle the so-called traditional idealisations (also called Galilean idealisations) some of which we saw in Maddy’s list and which seem to be coextensive with the physical idealisations I described above.

So Batterman *is* committed to the existence of *sui generis* mathematical idealisations, idealisations which he believes to be genuinely explanatory parts of empirical explanations. Just as I avoided examining how many categories of physical idealisation there are I shall also avoid investigating here whether all traditional idealisations are what I have called physical idealisations, but this seems to me a plausible hypothesis at least as far as the list above is concerned, and so I suggest tentatively that ‘abstracting’ and ‘restructuring’ idealisations are all the traditional idealisations that there are. I shall divide possible idealisations into two putative classes, physical/traditional/Galilean and mathematical/non-traditional/non-Galilean. This is an adequate framework within which to conduct the enquiry that follows, where I argue, *inter alia*, that we are justified in making many physical idealisations because experience shows that the relevant (i.e. result-affecting) differences between successfully idealised and non-idealised situations are minimal and don’t affect the usefulness of the idealisation. If there are too many important differences between the idealised and non-idealised situation then the idealisation simply won’t work, a fact that will become apparent when predictions based on the idealisation are compared with actual empirical data. It is plausible, I contend, that the sort of ‘mathematical’ idealisations Batterman has in mind are simply too divorced from the empirical phenomena they model to be considered proper idealisations.

6.4.2. Mathematical Idealisations, Batterman, and the Mapping Account.

We must now try to answer the question of whether there are what Batterman calls mathematical idealisations, and if there are, can they be genuinely explanatory? The first question would be decided in the positive if an example of what a mathematical idealisation could be found, the second if it could be shown that the example is really an explanatory one. Batterman, in the above-mentioned paper does indeed try to provide details of such an example. It is not entirely clear in that paper whether he is intending the case discussed to be one single extended example, or whether it is to be a variety of such examples set in a similar context. He provides a relatively scant discussion of the example in question, focussing more on the consequences it would have for the mapping account. My knowledge of thermodynamics is admittedly somewhat limited, and this part of Batterman's paper is somewhat dense, but I shall first draw out the example I interpret him as seeking to provide and then restate the example as clearly and concisely as possible afterwards, in order to facilitate my response to it.

The general tenor of Batterman's discussion concerns that fact that to explain phase transitions of fluids (from e.g. liquid to gas when we boil a kettle) using statistical mechanics, we have to invoke the 'thermodynamic limit', which is only reached when the number of particles in the system is infinite. This limit is required because for a system of "a finite number of particles the statistical-mechanical analogues of the thermodynamic functions cannot exhibit the non-analytic behaviour necessary to represent the qualitatively distinct behaviours we observe" (Batterman, 2010 p.8). So we have to force the functions to exhibit the non-analytic behaviour we require them to possess, and this forcing is achieved by invoking the thermodynamic limit. When the thermodynamic limit is reached statistical-mechanical functions do exhibit the non-analytic behaviour required to represent a phase transition. Since any particular thermodynamic system will consist of only finitely many particles the thermodynamic limit, with its infinity requirement, is obviously an idealisation. The nature of this idealisation might well at first seem counterintuitive since surely to describe a complicated system it makes more sense

to try to reduce its complexity rather than increase it, since such a reduction would have the effect of decreasing the number of variables involved in a calculation of the behaviour of the system, and subsequently the idealised system could be approximated to the real system within a certain margin of error. Such a thought is overly simplistic however, as simplifying the treatment of a system by viewing it as containing more objects than it in fact does is fairly common, as Maddy's water wave example shows.

So the increase in complexity of the system is what enables a scientist to make the calculations concerning phase transitions at all. This is because reaching the thermodynamic limit of a system, that is, by treating it as if it were infinite, we can describe that system's phase transitions using statistical mechanics rather than some sort of continuum thermodynamics precisely because this idealisation permits the statistical mechanical functions to exhibit non-analytic behaviour. Specifically, once the thermodynamic limit is reached, one of the statistical-mechanical functions, the free energy function, returns a singularity for certain values of the system, and each singularity corresponds to a phase transition. This is the core of Batterman's example, that *we make a false infinite assumption about a system in order to permit the taking the thermodynamic limit, a mathematical operation, so that the free energy equation can go to a singularity when it takes certain values, namely those corresponding to a phase transition and that this is the explanation, or an indispensable part of the explanation, of phase transitions* – even though according to the equations, phase transitions “do not exist at all in finite systems! They appear only in the thermodynamic limit” (Styer 2004, p.27) when certain thermodynamic statistical-mechanical functions return a singularity. The thermodynamic limit is as follows: “the statistical free energy should be identified completely with the thermodynamic free energy only in the limit that the system becomes infinitely large whilst the density...approaches a finite value” (Fisher 1964 p.377).

Batterman is somewhat more concerned with the details of the critical behaviour of a thermodynamic system, although the discussion of critical behaviour is not needed to further Batterman's point. I will however mention some of the particulars here, for completeness: when you boil a kettle you will find that at the

point of boiling there is both liquid and gas in the kettle, as not all of the molecules will have changed phase. However if the temperature and then the pressure are increased beyond the critical point needed for the phase transition (dependent on the fluid in question), and then the temperature drops below that needed for the transition, according to the equations the fluid changes from gas back into liquid without there ever being a gradual state where there is both some liquid and some gas, as would happen when we boil the kettle. The liquid-to-gas change is gradual and on a particle-by-particle basis, whilst the change from gas to liquid at the critical point is instantaneous for the fluid as a whole. This instantaneous change is the *critical behaviour*. Moreover this critical behaviour is universal because all fluids, no matter how physically dissimilar (some of the fluid systems concerned even include the magnetic) exhibit this critical behaviour with respect to the temperature decrease and all the curves of the graphs depicting this critical behaviour in each fluid are the same shape. But to reiterate, I think Batterman's point is served if we simply bear in mind what was summarised in the previous paragraph. Too much thermodynamic detail in this instance only obscures the philosophy, since the concern of this thesis is with philosophy of mathematics not philosophy of physics.

Why should we think that Batterman's mathematical idealisation example is substantially different from a physical idealisation example given by Maddy, whereby in the physics of wave phenomena in fluids it is assumed that the fluids are infinitely deep? In both cases a false assumption about the infinity of the system is made. The reason is that in the water wave case the infinity assumption only simplifies the equations, which would be much longer, and contain a great many more terms, without it. In contrast, in the thermodynamic instance the assumption is necessary in order to make the statistical mechanical explanation possible, since without it the explanation is not just more abstruse but non-existent. I am inclined to paraphrase Craig Callender (2001) and assert that to agree with Batterman here is to 'take statistical mechanics too seriously'. This opinion notwithstanding I do agree with Batterman it is an interesting example. However I believe he jumps far too quickly to the conclusion that the example genuinely captures or explains what is going on empirically speaking. As physicists (and even Batterman) accept, nature

is both finite and singularity free. So how could it be that a false description of a system under conditions that cannot actually obtain, yielding phenomena that cannot exist, can *actually explain* phase transitions, a real process we observe, yet which the equations indicate we should not, in our finite-particle systems, observe?

After all, it is not as if Batterman is saying that ‘there really are singularities doing explanatory work, and thermodynamic systems really are infinite, only we don’t observe either of these’. In the absence of a rigorous account of how the thermodynamic limit is genuinely explanatory, or indeed how non-causal explanations can be genuinely explanatory in physical systems, we must instead conclude simply that statistical mechanics is not adequate for the description of all thermodynamic phenomena. It may yield useful results and accurate predictions, but it is not *explaining* the phenomena since it is forced to describe. *For how are we to escape such a conclusion if the only way to make statistical mechanics work even in principle is to make assumptions that are very obviously false*, viz. that all thermodynamic systems experiencing phase transitions contain an infinity of particles?⁷⁵ Clearly something is going on here, something no scientist can ignore, namely that there is some connection between the singularity in the function and the phase transition in the fluid, but to conclude that the thermodynamic limit and the associated apparatus is genuinely explanatory seems much more absurd than concluding that our understanding of the phenomena, and our equations describing them, are imperfect and incomplete and that statistical mechanics is only part of the story.

For those unconvinced by the last few paragraphs, I must present this demand: explain *how* this apparatus could be physically explanatory. Clearly an

⁷⁵ Batterman does explicitly deny that all physical explanations are causal (2010, p.2) for the reason that the better explanations may involve eliminating causal details. However he has failed to observe that what is a better explanation for a scientist may indeed be a less genuine explanation, a concept developed in chapter three, simply because understanding the genuine explanation at a sufficient level of detail may be too difficult for the scientist to develop or comprehend, and a non-genuine explanation works well enough for predictive or general scientific purposes. In some case the scientist will know his explanation is not genuine, as when an engineer uses Newtonian mechanics in his work, and in other cases he will be unaware, as Newton was when he developed his mechanics. So it is clear that Batterman’s reflection on causal explanation has missed the point here and does not support the idea that there are non-causal empirical explanations in any way.

explanation of phase transitions is required, but we need to be clear about what we are explaining. For instance, we do not need to explain critical behaviour as it was outlined above since the actual phase-transition is not in fact instantaneous at all, but is rather an idealisation concerning the fact that phase transitions happen *very* fast. But nevertheless this incredible speed of phase transition of a system dropping below the critical point does need explaining, though we must accept that we may not have the theoretical apparatus required to do so currently. The thermodynamic-limit-based explanation may give the right results but it is quite clearly not doing so for the right reasons, or at any rate, if in some sense it is capturing what is going on empirically, we need a clear account of why. Either way, without more information the thermodynamic-limit explanation as it stands is incomplete, and Batterman's example is left without very much force.

I realise I could be seen to have left myself open to a possible counter-response by Batterman, namely that I am confusing platonistic entities and mathematical operations, the very distinction with which he opens his paper, and that I am claiming that the thermodynamic limit is not genuinely explanatory because it is some sort of abstract object, or even some sort of platonistic concept, a view I advocated in chapter three. But this is not the case. My concern is not that the thermodynamic limit is abstract or otherwise, but that the conditions for the limit (or some possible empirical analogue to it) to be reached are false of any thermodynamic system found in nature. I have no objection to the idea that platonistic operations (or indeed nominalistic operations, as in the geometric concept of primeness given in chapter three) operations can be components of physical explanations, so long as those operations are representing *empirical* operations relevant to the phenomenon being explained. As such talk about adding lengths, or juxtaposing intervals, can be understood to be representing the concatenation of objects.

Using mathematical operations is a very convenient and natural way of talking about possible empirical operations, and enables us to focus on the structural properties of systems, but all that is going on is that mathematical operations *represent* possible empirical operations. So in principle I have no

objection to Batterman's idea that mathematical operations can be explanatory, assuming that the mathematical operations in question are empirically instantiated, empirically realised, that they have an empirical content which is actually doing the explaining. A good argument will need to be forthcoming to establish that they can be explanatory without such analogues, and Batterman's example does not establish such an argument. For Batterman has not shown that the taking of the thermodynamic limit has a physical analogue, and neither has he shown that to be explanatory it does not require one.

Batterman, as I said above, does agree that the mapping account is sufficient to handle 'traditional' idealisations (Batterman 2010 p.17), and he admits "[s]urely something is right about the mapping account. In particular, when it comes to representing physical structures, mathematical structures often provide useful models..." (Batterman 2010 p.11), but he maintains vigorously that it cannot handle non-traditional, or mathematical, idealisations which he feels play an explanatory role in empirical explanations. He claims "all these [mapping] approaches fail to deal with the most difficult features of mathematical explanation...how mathematical idealisations can have a role in physical explanations" (Batterman 2010, p.2). That is, he believes the mapping account does not explain *why* the thermodynamic limit explains phase transitions. I have obviated this need by arguing that the thermodynamic limit does not really explain such transitions, and so there is nothing for the mapping account to explain.

Batterman's paper is a commendable attempt to challenge the mapping account in a non-question-begging fashion, but one that I have argued, ultimately fails. To summarise, allow me to answer the earlier questions: the consequences of the existence of such idealisations would be catastrophic for the mapping account, but since there is no good evidence for such idealisations the mapping account theorist is in the clear and can construe all genuine idealisations as physical idealisations, although such idealisations are not necessarily exhausted by the two categories listed above. We can now turn to the next issue, namely how should the mapping account theorist give an account of even physical idealisations? Indeed is such an account required at all?

6.4.3 The Mapping Account and Physical Idealisations

The issue of idealisations has stirred a lot of debate in the philosophy of science and the philosophy of applied mathematics, and no one would want to assert that the mapping account is entirely divorced from any considerations of idealisations, for as we saw with Batterman, the existence of certain *mathematical* idealisations would have negative consequences for that account. But does this mean that the mapping account needs to trouble itself with accommodating *physical* idealisations, or developing a working theory of them? In a physical idealisation we make a false assumption about a physical system in order to simplify calculations about it, calculations whose effectiveness and applicability are fully explained by the mapping account, or so I argued in chapter two. That is, we treat the physical system as different from how it actually is – and then we apply the mapping account. We don't apply the mapping account and then make the idealisation.

So it is not clear that it is the role of the mapping account theorist to say anything more about the issue of idealisations, which seem to precede its application. That is not to say that physical idealisations are not an interesting object of study, and of course the philosopher of science will need to trouble himself with how it is that idealisations work. But once we have a working theory of physical idealisations the mapping account theorist (and any philosopher of applied mathematics of any persuasion whatsoever within reason) can help himself to it. To deny this would be tantamount to asserting that the mapping account needs to explain everything that the user of such an account also makes use of – so why not demand of such an account that it explain mathematical truth, how analysis works and the meaning of limit as well? Such a demand would be misguided, for clearly the mapping account theorist can help himself to many pre-existing theories of such issues, and there is no good reason why the same should not be the case with idealisations. Despite this, I shall say a word about physical idealisations and why we can analyse some idealisations in terms of partial mappings.

The issue is: what exactly is going on when we make a physical idealisation? The idealisation is made in order to arrive at a description of a situation that is

sufficiently similar to a description of another situation such that facts concerning the first situation also apply to the second situation, within a certain margin of error or a certain degree of approximation. The degree of approximation required depends on the situation at hand. For instance, in the example of calculating how many people are required to pull a block up a ramp, it is possible to ignore certain forces if they are sufficiently small. Say that friction is so minimal that its inclusion in the calculation will not result in any extra people being predicted to be required. Then the idealisation is acceptable – we have performed a simpler calculation with fewer forces, and obtained a result that is sufficient for e.g. the effective allocation of the labour force. But the similar idealisation would not be acceptable in the analysis of the behaviour of subatomic particles in a particle accelerator, where no force could be insignificant.

What happens in the example just given is that we describe a situation which in fact does not obtain, in this case a situation of a block being pulled up a ramp where there is no friction (a fictional situation, you might say) and conclude x amount of people are needed (on average) to pull the block up the ramp. Then we take a situation that does obtain, namely a situation similar except that here friction is a factor, and argue that for such and such an empirical reason the result from the fictional case is applicable to the actual case. The example with the water waves is similar, although this involves not the removal of an element that should be integrated into the calculation but rather a totally false picture of what is going on. In this case we perform calculations with the simplifying assumption that the water is infinitely deep, and then argue that for various empirical reasons the result of such a calculation is applicable to the case where the water is not infinitely deep. As I am not a wave mechanic I am not qualified to stipulate what such empirical conditions would be, but they must obtain in order for the idealisation to be useful at all. For idealisations are judged only by their usefulness and appropriateness to describe, approximately, a given system.

The ability of mathematics (platonistic or otherwise) to represent actual, possible and fictional systems makes an idealisation possible, but the mapping account theorist does not need to give an account of idealisation beyond a general

explanation of the representational power of mathematics, which was already achieved in chapter two. The utility of the idealisation consists not in the existence of mappings, partial or full, between the idealised system, a mathematical structure, and an actual system, but rather in the similarity of the idealisation and the actual system within certain independently determined parameters. The mapping theorist needs to explain how mathematics can represent what is going on in the idealised system in such a way that (counterfactually) accurate, true-in-the-story-of-the-idealisation results and predictions concerning that idealisation are obtained, and he needs to explain how mathematics can represent what is going on in the actual system in such a way that accurate results and predictions concerning *that* system are obtained. This was done in chapter two. But there is no reason why he has to account for how the idealisation and the actual system relate, for such considerations go beyond the purview of the mapping account insofar as the matter is not one of the representational power of mathematics but rather the empirical properties of empirical and idealised systems and their similarity to each other.

If there were such a thing as a mathematical idealisation in a genuine empirical explanation then it is true the mapping account would have to address that but I have already argued that such idealisations do not exist, at least not in any pernicious form, and so the mapping account theorist can leave discussions of idealisations to the philosopher of science, to whose domain they have traditionally belonged. Despite these considerations, some philosophers of mathematics have tried to give an account of idealisation within the framework of a mapping account, e.g. Bueno and Colyvan and their account of partial mappings which was briefly encountered in chapter two. As I said then, and repeat now, if invoking partial mappings is of some explanatory value to the mapping theorist, then there is no reason why he should not help himself to the notion. But given that I have just criticised the need for the mapping account theorist to give a treatment of physical idealisation at all, this would seem to be misguided.

6.4.4 Idealisations in Field and Rizza

Idealisations are frequently made in the work of Field and subsequently of Rizza. I want to briefly consider these here in order to ascertain if they are merely physical idealisations, which I feel have been adequately accounted for, or some other type of idealisation which is potentially harmful to the nominalist version of the mapping account, which gets off the ground partly due to these idealisations. Rizza, for instance, does say “I am interested...in mathematical nominalism rather than nominalism *tout court*. For instance I accept making use of idealized descriptions of physical settings when making references to abstract objects” (Rizza 2010 p.53). He continues (ibid.) “In this sense my position is in line with the one expressed in Field (1980 p.31)”. I do not agree that as it stands this is an acceptable position, since surely nominalism *tout court* is the motivation for mathematical nominalism, at least if we want to avoid charges of arbitrariness which could be raised in response.

So if Rizza – and Field – are to be vindicated it is essential to show that the sort of idealisations they use can be treated as involving no reference to such nominalistically unacceptable objects. The major ‘idealisation’ made by Field, which indeed makes many of his representation theorems possible at all, is the assumption that physical space is continuous. This is considered objectionable by philosophers believing that “there doesn’t seem to be a very significant difference between postulating such a rich physical space and postulating the real numbers” (Field 1980 p.31). Field’s response is that “postulating uncountably many physical entities is not an objection to nominalism; nor does it become any more objectionable when one postulates that these physical entities obey structural assumptions analogous to the ones platonists postulate for real numbers” (Ibid.), a view consistent with the approach to nominalism taken through this thesis, viz. that nominalism is an issue of abstractness and not structure or quantity. Thus, Field’s treatment of space-time is not really an idealisation but rather an empirical assumption.

An interesting question is the degree to which Field’s nominalism and indeed the nominalised version of the mapping account itself has to depend on such an empirical assumption, whether there is any way of reformulating it such that it

would be workable if space-time points were denumerable or even finite. I do not have space to address this issue here. But I do not regard it as an especially contentious view that philosophy should depend in some way on what is true empirically. In the same way, I presume most physicalists in the philosophy of mind would regard the truth of a given version of physicalism as an empirical question, one which is nevertheless answerable in part by using the methods of philosophy and not just of natural science. So much for Field's idealisations. What of Rizza's? Rizza seems concerned about one particular use of idealisation, that involved with c-types. He does mention the abstractions involved in physical idealisation, and suggests that "such description has only to do with the idealisation of a concrete setting and does not by itself support commitment to a distinctively mathematical ontology" (ibid. p.56). This is a view which I argued broadly for above. But with c-types the issue seems different. We are not idealising our empirical description in order to apply the measurement system, rather the measurement system itself seems somewhat idealised, since as Rizza admits,

the approach I have presented only involves talk of empirical settings and observational reports generated by physical interactions occurring within them. Of course a considerable degree of idealization is involved, as only finite fragments of C-types are obtained through actual experimental procedures. (Rizza 2010 p.67).

The problem is that we obviously act as though there is a continuous measurement scale when only part of the scale has been generated. But I submit this need not be especially contentious if we are free to admit that although only some c-types are generated, the structural properties that the class of all c-types would have if they all existed are clear, and we can speak perfectly easily about possible c-types as possible values of measurement, although these may or may not be nominalistically acceptable. But that is a question for another essay.

6.5 Thesis Conclusion

The primary purpose of this thesis has been to defend a representational conception of applied mathematics and to argue for a mapping account of applicability. This is an account which has been advocated, in varying degrees of detail, by a variety of philosophers, a fact which raises two questions regarding my contribution: firstly, why should a discussion of a mapping account receive a thesis-length treatment? Secondly, what has my discussion contributed over and above the contributions in the rest of the literature that suggest it may have any philosophical value? The first question is the easiest to address. For bear in mind the facts of the matter: we do apply mathematics to the physical world all the time, and in an extremely useful way. Whether we think mathematics is about abstract objects, or about nominalistically acceptable structures, this is a fact that clearly stands in need of explanation. The task of explaining the applicability of mathematics to the physical world falls to neither natural science nor mathematics, but rather philosophy. The reason that such an extended treatment of the mapping account is required is simply that for such an important question, relatively little attention has been devoted to it, and the attention that it has received is generally both scant (although this situation has changed somewhat in recent years) and brief (no treatment is longer than an average-sized article) with little or no analysis of many of the philosophical consequences of such an account. So a more in-depth treatment was both timely and necessary.

But what of the second question? What has my discussion contributed to the debate beyond what is already contained in the various articles about, and occasional book relevant to, the representational conception and mapping account? I think the answer to this question lies in several areas, three especially. The first is the detailed look in chapter two at the relation of the mapping account to derived attributes and natural laws, which I have not observed anywhere else. The second is the considerations about what exactly the mapping account has to explain, focused on the dispensability of platonistic explanations of empirical phenomena in chapter

three and the attack on Steiner's position in chapter five. Thirdly, in chapter six I uncovered the ontological ramifications of the account and discovered that the mapping account is not bound to include a treatment of physical idealisations.

My general strategy was to develop the mapping account in detail whilst defending a representational conception of applied mathematics in general. Thus in chapter four I looked at the ways to avoid formulating scientific theories in platonistic terms in some detail because of the indirect support this provided for a representational conception of applied mathematics in which platonistic mathematics did not play an essential explanatory role in empirical explanations. In chapter five I addressed a very serious challenge that has been raised by Mark Steiner, which if successful would show, despite the considerations in chapters three and four, that the role of mathematics is not purely representational. I categorised the types of challenge which Steiner made, and attempted to refute each category individually, arguing that the mapping account can meet, or is consistent with views that do meet, the issues that make up the descriptive problem.

For the bulk of the thesis I was keen to keep the mapping account as 'ontology neutral' as possible. Beyond the references to functions, real numbers, and other abstract mathematical objects, and some scepticism concerning genuine platonistic explanations of empirical phenomena, no explicit mathematical ontology was endorsed. This was to avoid getting bogged down in intractable metaphysical matters at an early stage, and to assess the mapping account independently of these metaphysical concerns, a vital move if the account was to be clearly developed. I have made clear that I believe a nominalist approach to mathematical ontology is the correct one, and suitably reinterpreted the mapping account will provide an excellent account of some nominalistically acceptable mathematics-surrogate to empirical phenomena. But if it turned out that platonism is true then the mapping account is an excellent explanation of the applicability of mathematics, and still avoids commitment to genuine platonistic explanations of empirical phenomena, and any other view of the applicability of mathematics that is not purely representational.

Mathematics is the language with which we both describe the wonders of the cosmos and measure the mundane. If we do not understand how mathematics can be applied our understanding of the universe will be impoverished. I have argued that the mapping account is the simplest and the best explanation we have. Newton said “truth is ever to be found in simplicity, and not in the multiplicity and confusion of things”, and I have shown that if the mapping account has one virtue it is a uniform treatment of the applications of mathematics, and that a representational conception of applicability reduces the confusions that result from having a piecemeal and gerrymandered theory of the applied mathematics. As I have presented it, the mapping account has been defended against the slings and arrows of its detractors, providing a firm and unequivocal understanding of applicability that is essential for a good deal of future work in the philosophy of mathematics.

Bibliography: Works Cited and Consulted

- Adams, E. (1966) 'On the Nature and Purpose of Measurement', *Synthese*, vol.16, pp. 125-169.
- Aristotle (2001) *The Basic Works of Aristotle*, Random House: New York.
- Aristotle (2001a) *Physics*, in Aristotle, *The Basic Works of Aristotle*.
- Aristotle (2001b) *De Caelo*, in Aristotle, *The Basic Works of Aristotle*.
- Armstrong, D. M. (1978a) *Nominalism and Realism (Universals and Scientific Realism Vol. 1)*, Cambridge: Cambridge University Press
- Armstrong, D. M. (1978b) *A Theory of Universals (Universals and Scientific Realism Vol. 2)*, Cambridge: Cambridge University Press
- Armstrong, D. M. (1983) *What is a Law of Nature*, Cambridge: Cambridge University Press
- Baker, A. (2005) 'Are There Genuine Mathematical Explanations of Physical Phenomena?', *Mind*, vol. 114, pp. 223-238
- Baker, A. (2009) 'Mathematical Explanation in Science', *British Journal for the Philosophy of Science*, vol. 60, pp. 611-633
- Baker, A and M. Colyvan (2011) 'Indexing and Mathematical Explanations', *Philosophia Mathematica*, vol. 19, pp. 323-334.
- Balaguer, M. (1996) 'Towards a Nominalisation of Quantum Mechanics', *Mind*, vol. 105, pp. 209-226
- Bangu, S. (2006) 'Steiner on the Applicability of Mathematics and Naturalism', *Philosophia Mathematica*, vol.14, pp. 26-43.
- Bangu, S. (2008) 'Inference to the Best Explanation and Mathematical Realism', *Synthese*, vol. 160, pp. 13-20
- Bar-Hillel, Y. (Ed.)(1965) *Logic, Methodology and Philosophy of Science: Proceedings of the 1964 International Congress*, Amsterdam: North-Holland
- Batterman, R. (2010) 'On the Explanatory Role of Mathematics in Empirical Science', *British Journal for the Philosophy of Science*, vol. 61 pp. 1-25.
(note pagination in this thesis refers to the version from Batterman's website).
- Berker, K. (1983) *Measurement: Its Concepts, Theories and Problems*, Holland: D. Reidel.
- Boolos, G. (1984) 'To Be is To Be the Value of a Variable, or the Value of Some Variables', *Journal of Philosophy*, vol. 81, pp. 430-449.
- Bueno, O, and M Colyvan (2011) 'An Inferential Conception of the Application of Mathematics', *Nous*, vo. 45, pp. 345-374.
- Bueno, O, S French & J Ladyman (2002) 'On Representing the Relationship Between the Mathematical and the Empirical', *Philosophy of Science*, vol. 69, pp. 497-518

- Burgess, J. (2004) 'Quine, Analyticity, and Philosophy of Mathematics', *Philosophical Quarterly*, vol. 54, pp. 38-55
- Burgess, J and Rosen, G. (1997) *A Subject with no Object: Strategies for Nominalistic Interpretation of Mathematics*, Oxford: OUP
- Callender, C. (2001) 'Taking Thermodynamics Too Seriously', in *Studies in History and Philosophy of Science Part B*, vol. 32, pp. 539-553.
- Chihara, C. (1990) *Constructability and Mathematical Existence*, Oxford: OUP
- Church, A. (1956) *Introduction to Mathematical Logic, Vol.1*, Princeton NJ: Princeton University Press
- Clark, R. (2004) *Representation and Mathematical Applicability*, MPhil Thesis: King's College London
- Colyvan, M. (2000) 'Review of Steiner 'Applicability of Mathematics as a Philosophical Problem1'', *Mind*, vol. 109, pp. 390-394.
- Colyvan, M. (2001) *The Indispensability of Mathematics*, Oxford: OUP
- Colyvan, M. (2001a) 'The Miracle of Applied Mathematics', *Synthese*, vol. 127, pp. 265-277
- Colyvan, M. (2002) 'Mathematics and Aesthetic Considerations in Science', *Mind*, vol. 111, pp. 69-74
- Colyvan, M (2010) 'There is No Easy Road to Nominalism', *Mind*, vol. 119, pp. 287-306
- Cooley, J & M Marshal (2000) 'Periodical Cicada Homepage', <http://insects.ummz.lsa.umich.edu/fauna/>, University of Michigan, Insect Division.
- Dretske, F. (1977) 'Laws of Nature', *Philosophy of Science*, Vol. 44, pp. 248-268
- Enderton, H. (2009) 'Second-Order and Higher-Order Logic', <http://plato.stanford.edu/entries/logic-higher-order>
- Feynmann, R. (1965) *The Character of Physical Law*, London: Penguin.
- Field, H. (1980) *Science Without Numbers*, Oxford: Blackwell
- Field, H. (1981) 'Realism and Anti-Realism about Mathematics', in Field, *Realism, Mathematics and Modality*
- Field, H. (1984) 'Can we Dispense with Space-Time?', in *Proceedings of the Biennial Meeting of the Philosophy of Science Association*, 1984, vol. 2, pp. 33-90.
- Field, H. (1985) 'On Conservativeness and Incompleteness', in Field, *Realism, Mathematics and Modality*.
- Field, H. (1989) *Realism, Mathematics and Modality*, Oxford: Blackwell
- Fisher, M. (19 'The Free Energy of a Macroscopic System', *Archive for Rational Mechanics and Analysis*, vol. 17, pp. 377-410
- Frege, G. (1884) *The Foundations of Arithmetic*, Evanston IL: Northwestern University Press

- Frege, G. (1891) 'Function and Concept', in Frege, *Translations From The Philosophical Writings of Gottlob Frege*.
- Frege, G. (1892) 'On Concept and Object', in Frege, *Translations From The Philosophical Writings of Gottlob Frege*.
- Frege, G. (1980) *Translations From The Philosophical Writings of Gottlob Frege*, 3rd Edn, Totowa NJ: Barnes and Noble
- Friedman, M (1981) 'Review of *Science without Numbers*', *Philosophy of Science*, vol. 48, pp. 505-506
- Hand, M. (2004) *Measurement Theory and Practice*, London: Arnold
- Hellman, G. (1989), *Mathematics without Numbers*, Oxford: OUP
- Hellman (2005) 'Structuralism', in *The Oxford Handbook of Philosophy of Mathematics and Logic*.
- Holman, E.W. (1969) 'Strong and Weak Extensive Measurement', *Journal of Mathematical Psychology*, vol. 6, pp. 286-293.
- Hossack, K. (2000) 'Plurals and Complexes', *British Journal for the Philosophy of Science*, vol. 51, pp. 411-443.
- Hughes, G. and Cresswell, M. (1966) *A New Introduction to Modal Logic*, London: Routledge
- Krantz, D.H., R.D Luce, P. Suppes & A. Tversky (1971) *Foundations of Measurement, Volume One*, New York: Dover.
- Kuhn, T.S. (1996) *The Structure of Scientific Revolutions*, (1st edn. 1962), Chicago IL: Chicago University Press
- Kyburg Jr., H.E. (1997) 'Quantities, magnitudes and Numbers', *Philosophy of Science*, vol. 64, pp. 377-410.
- Lyon, A and M. Colyvan. (2008) 'The Explanatory Power of Phase Spaces', *Philosophia Mathematica*, vol. 16, pp. 227-243.
- MacBride, F. (1999) 'Listening to Fictions: A Study of Fieldian Nominalism', *British Journal for the Philosophy of Science*, vol. 50, pp. 431-455.
- Maddy, M. (1997) *Naturalism in Mathematics*, Oxford: OUP
- Malament, D. (1982) 'Science Without Numbers: A Defence of Nominalism, by Hartry Field', *Journal of Philosophy*, Vol. 79, pp. 523-534.
- Melia, J. (2000) 'Weaseling Away the Indispensability Argument', *Mind*, vol. 109, pp. 455-479
- Melia, J. (2002) 'Response to Colyvan', *Mind*, vol. 111, pp.75-79
- Niederee, R. (1992) 'What do Numbers Measure?', *Mathematical Social Sciences*, vol. 24, pp.237-276.
- Ostrom, JH. (1974) 'Archaeopteryx and the Origin of Flight', *Quarterly Review of Biology*, vol. 49, pp. 27-47.
- Penrose, R. (1989) *The Emperor's New Mind*, London: Vintage

- Pincock, C. (2004a) 'A Revealing Flaw in Colyvan's Indispensability Argument', *Philosophy of Science*, vol. 71, pp. 61-79
- Pincock, C. (2004b) 'A New Perspective on the Problem of Applying Mathematics', *Philosophia Mathematica*, vol. 12, pp. 135-161.
- Planck, M. (1981) *Where is Science Going?*, Woodbridge CT: Ox Bow Press.
- Putnam, H. (1979) *Mathematics, Matter and Method*, Cambridge: CUP
- Resnik, M. (1997) *Mathematics as a Science of Patterns*, Oxford: OUP
- Rizza, D. (2010) 'Mathematical Nominalism and Measurement', *Philosophia Mathematica*, vol. 18, pp. 53-73.
- Rizza, D. (2011) 'Magicality, Mathematical Explanation and Mathematical Realism', *Erkenntnis*, vol. 74, pp. 101-114.
- Roberts, F. (1979) *Measurement Theory*, Reading MA: Addison-Wesley.
- Roberts, F and D. Luce. (1968) 'Axiomatic Thermodynamics and Extensive Measurement', *Synthese*, vol. 18, pp.311-326.
- Saatsi, J. (2007) 'Living in Harmony: Nominalism and the Explanationist Argument for Realism', *International Studies in the Philosophy of Science*, vol. 21, pp. 19-33. (However page references in this thesis refer to the preprint at http://www.personal.leeds.ac.uk/~phljts/Juha_Saatsi/Research_files/Living.pdf)
- Shapiro, S. (1997) *Philosophy of Mathematics: Structure and Ontology*, Oxford: OUP
- Shapiro, S. (2005) *The Oxford handbook of Philosophy of Mathematics and Logic*, Oxford: OUP
- Simons, P. (2001) 'Review of Steiner "The Applicability of Mathematics as a Philosophical Problem"', *British Journal for the Philosophy of Science*, vol. 52, pp. 181-184.
- Steiner, M. (1998) *The Applicability of Mathematics as a Philosophical Problem*, Cambridge MA: Harvard
- Styer, D. (2004) 'What Good is the Thermodynamic Limit?', *American Journal of Physics*, vol. 72, pp. 25-29.
- Szczerba, L.W & A. Tarski (1964) 'Metamathematical Properties of Some Affine Geometries', in Bar-Hillel, *Logic, Methodology and Philosophy of Science*, pp. 166-178
- Tooley, M. (1977) 'The Nature of Laws', *Canadian Journal of Philosophy*, vol. 7, pp. 667-98
- van Fraassen, B. (1980) *The Scientific Image*, Oxford: OUP
- Weinberg, S. (1986) 'Lecture on the Applicability of Mathematics', *Notices of the American Mathematical Society*, Vol. 33
- Wigner, E. (1960) 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences', *Communications on Pure and Applied Mathematics*, Vol. 13, pp. 1-14.

